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The Journal of HEREDITY

A monthly publication devoted to Plant Breeding Animal Breeding and Eugenics

SCISSORS OR HEREDITY?
(See page 83)

Children of the Insane — **Chimeras in Figs**

Projects in Heredity — **Abnormal Zinnias**

Corn Improvement in Canada — **The Herediscope**

A Bequest to the A. G. A.

FEBRUARY, 1928 — Vol. 19, No. 2

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The

Journal of Heredity

VOL. XIX, NO. 2 FEBRUARY, 1928

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The JOURNAL OF HEREDITY is published monthly by the

AMERICAN GENETIC ASSOCIATION

At 1918-32 Harford Avenue, Baltimore, Md., and Victor Building, Washington, D. C.

Address communications regarding editorial matters and the business management of the Association to Room 306, Victor Building, Washington, D. C.

Entered as second class matter February 24, 1923, at the postoffice at Baltimore, Maryland. Acceptance for mailing at the special rate of postage provided for in Section 1103, Act of October 3, 1917, authorized December 20, 1918. **Contents copyrighted 1927 by the American Genetic Association. Reproduction of articles or parts of articles permitted only upon request, for a proper purpose, and provided due credit is given to the author and to the JOURNAL OF HEREDITY, Washington, D. C.**

Date of Issue of this number, March 19, 1928

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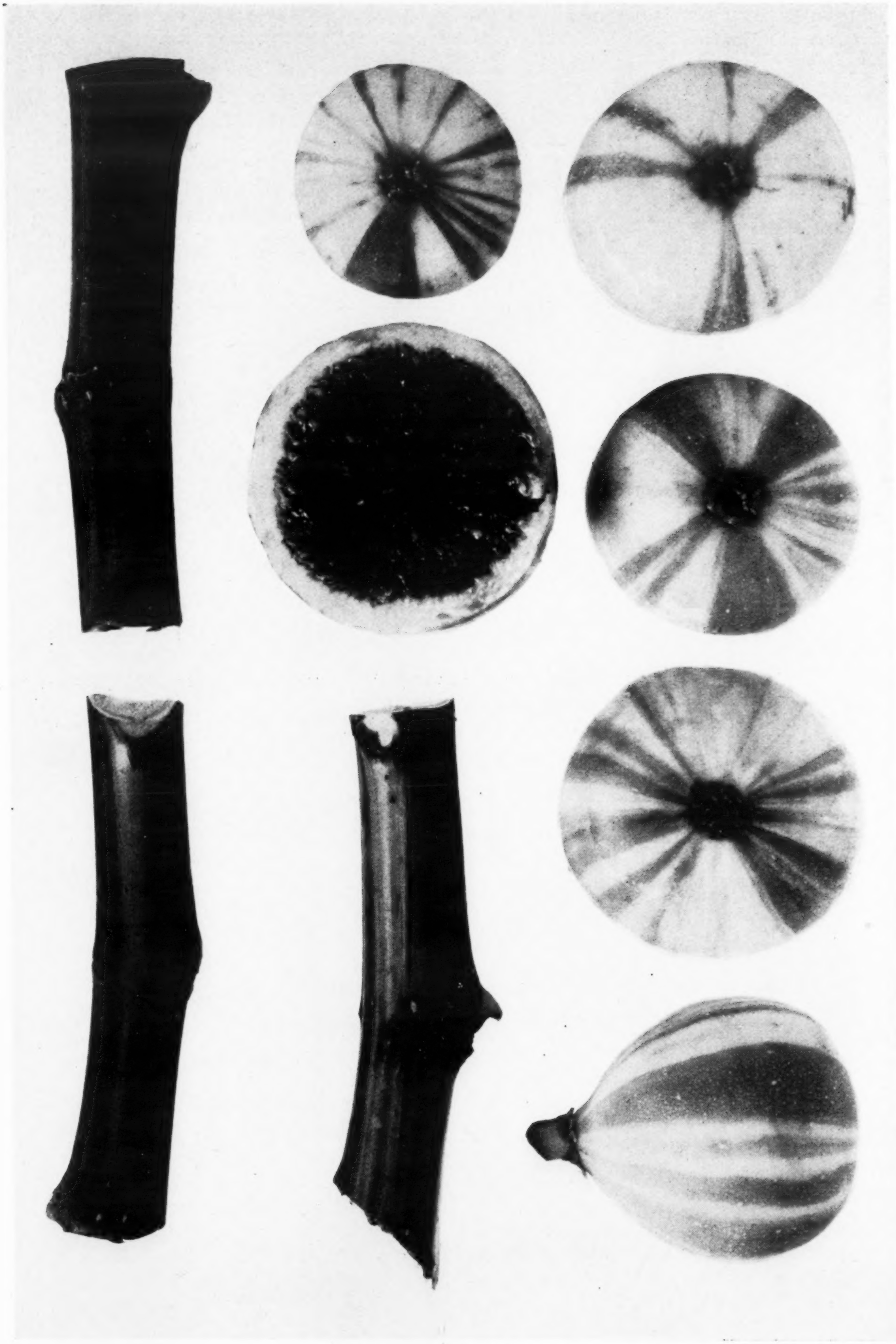


THE PANACHEE FIG

Frontispiece

The Panachee or "Tiger" Fig is a striking example of a color chimera which seems never to have been noted by writers on chimeras. The green fruits and, to a lesser extent the stems, are striped with varying proportions of green and yellow, but the leaves are solid green. The origin of the variety is not known, but it has been propagated at least since 1866.

OTHER FIG CHIMERAS

IRA J. CONDIT
Associate Professor in Subtropical Horticulture, University of California, Berkeley

ABOUT nine years ago J. L. Collins published in this Journal a short account of a color chimera in the fig with two illustrations furnished by the present writer. The photograph of the fruit showed a small sector of dark-colored skin contrasting strongly with the greenish-yellow of a normal Smyrna fig. The other showed two leaves of the Mission variety of fig, with sections almost pure white instead of green. The tree bearing these leaves was one of a half dozen similar trees found by chance in a nursery near Orange Cove, California. Each tree bearing variegated leaves was tagged and transplanted the following spring. Two set out near Fresno have made a very slow growth and no variegated leaves have so far appeared. The others, planted near Orosi, can not now be found.

Since 1919, other still more striking cases of chimeras in figs have appeared. The first to be mentioned is that of an Adriatic fig discovered in the Burleigh orchard near Fresno in 1922. As illustrated in Figure 2 this color chimera showed one-third of the fig to be a normal green and two-thirds dark, with this dark sector divided into two parts by a narrow ribbon of green. The bands of light and dark color persisted when the fig dried, and some of the unsophisticated judged it to be the result of a cross between the Black Mission fig and the White Adriatic. In this case also a tag was placed on the twig bearing the chimera but the tag was destroyed and the twig could not again be identified.

Another Adriatic twig on a tree near Merced has borne dark-colored figs each season for several years instead of normal green fruit. This sport has so far not been propagated.

A grower of the Calimyrna fig near Ceres reported a few years ago that one branch of a certain tree had produced banded fruits. He rooted a cutting from the branch but the plant later on perished.

The best example of a bud sport so far seen in a Calimyrna tree is in the Newby orchard, Reedley. A sucker had grown from one side of the trunk near the ground and eventually equalled the rest of the tree in height Variegated leaves appeared on certain twigs each year, some showing half yellow and half green, as in Figure 1, some entirely yellow, and others with various amounts of yellow and green. Those completely albino or chlorotic turned brown at the margins and died back early in the summer. While the leaf chimera in this case was striking, the fruit showed only faint indications of variegation.

The Panachee Fig

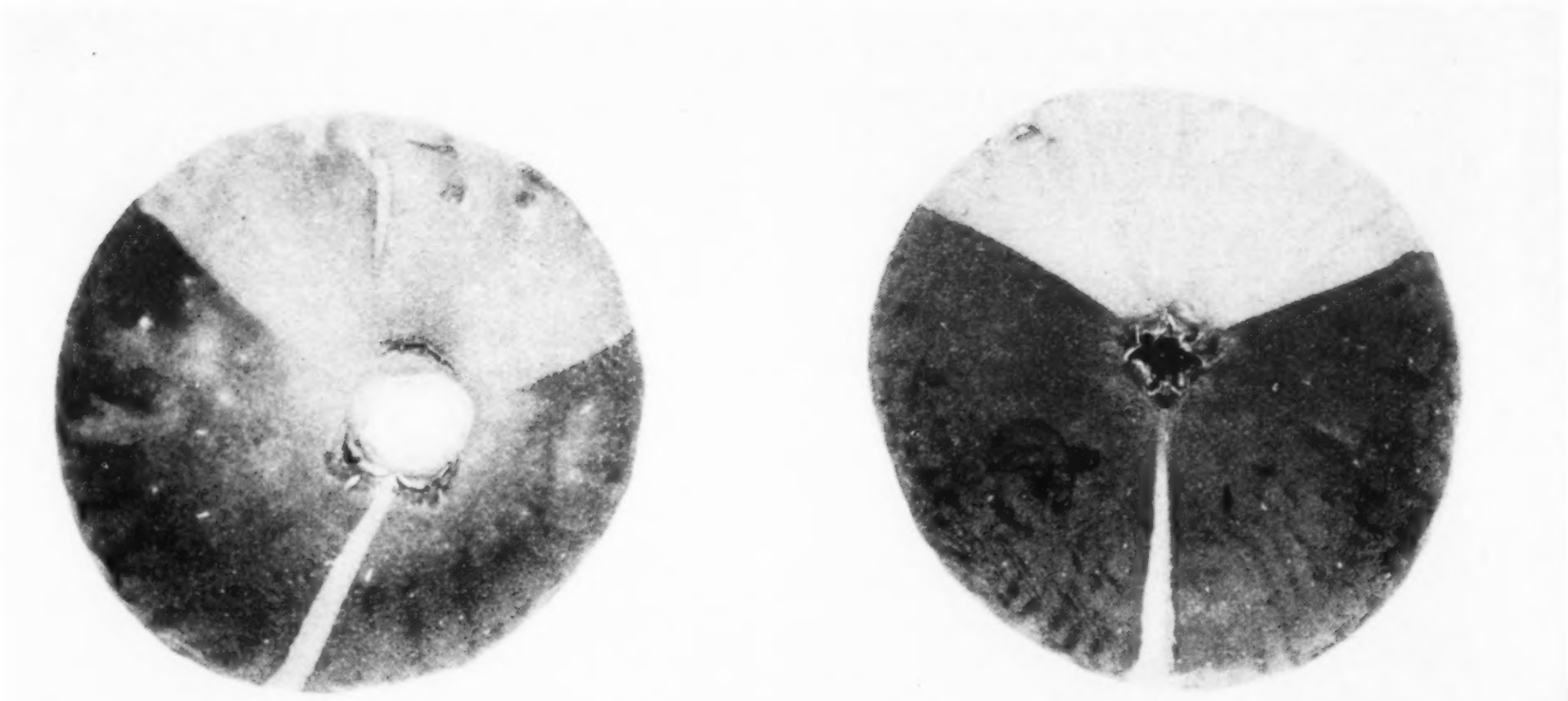
The most striking fig chimera, known to the writer, however, is that exhibited by the French variety Panachee. There are probably very few variegated fruits of any kind which equal this fig in distinctiveness of color bands and constancy of appearance of the variegation. While the variety has evidently been known in Europe and America for many decades, it appears to have escaped the notice of writers on chimeras and variegations, so far as we have been able to discover from a perusal of the literature, although it is described in horticultural texts.

Early reference to the Panachee is made by Hogg in 1866. He describes it as a fig "handsome in appearance; skin straw yellow, beautifully striped with longitudinal bands of bright lively green, some of which are broad and some narrow." No reference is made to the date of its introduction into

CHIMEROID LEAVES OF THE CALIMYRNA FIG

Figure 1

Variegated leaves similar to these are produced on certain twigs of a branch sport coming as a sucker from one side of the tree near the ground



A CHIMERA OF THE ADRIATIC FIG

Figure 2

Fruit from a tree near Fresno, California. The bands of light and dark color persist after the fig has ripened, in which it differs from the Panachee fig.

England or to the extent of its culture. A few years later, A. F. Barron concludes a list of fig varieties in England with "the striped-fruited fig (Figue Panachee) which is really very handsome and striking, being beautifully striped with deep green and pale yellow."

The same writer however in 1891 lists it as Col de Signora Bianca Panachee, a striped form of this well-known variety.

The Pomological Society of France included the Panachee in its list of cultivated figs for 1887, giving Pere Hilarion as a synonym. It was reported to bear one crop only. The skin was said to be streaked with bright yellow and green bands, without elevated ribs.

That the same or similar variety is found in Italy is evidenced by the account of Vallese in his book on the fig. He calls it "Fico variegato" from the Botanical Garden of Lecce, and gives two synonyms, "Fico fracazzano rigato, and Fico bracotedesco." The skin is described as having a yellowish white ground color, striped with pea-green bands, some narrow, some quite broad, extending to the peduncle or terminating at the scales; with the advancement of maturity, the green of these bands mostly disappears, becoming diffused with the yellow of the background.

Spanish writers fail to mention this variety among the figs grown in their country. At the Elsenburg Experiment Station, Cape Province, South Africa, it is grown under the name of Tiger.

The Panachee has been grown in the United States for over thirty-five years but neither the exact date of its introduction nor the individual responsible for obtaining it has been learned. Analyses of the fruit of this variety grown by Mr. George Roeding, Fresno, California, were made in 1892. The figs were described as banana yellow in color, striped lengthwise with light green bars.

In his catalogue and description of figs Dr. Gustav Eisen gives the following: "Panachee. (Striped.) Medium; round; skin bright, dark green, striped yellow. Pulp white. A very handsome striped fig, probably the

same as Jasper, with the description of which it agrees. Leaves and stems of these figs are distinctly striped." However, under Jasper he does not mention a variegated skin and adds that it is not identical with either Panachee or Col de Signora Panachee. In the description of the last-mentioned variety he follows that of Hogg almost word for word; he adds that it is not an extensively cultivated fig on account of inferior bearing qualities, but for a garden fig it is much to be recommended, as it would sell well in any market.

Starnes and Monroe include the Panachee among the figs grown in Georgia but describe it as apparently unsuited to the climate, being a very shy bearer and extremely late.

In 1914 Roeding listed it as Bourjassote Panachee and as a most peculiar but delicious fig. "Not only the branches of the tree are peculiarly marked, but the figs are splashed with green and yellow stripes. A fine table and market fruit."

According to Lawrence, the Panachee was being tested in Arizona in 1916 along with several other varieties.

In his account of variegated fruits published in 1922, Bunyard states that variegation occurs rather frequently in citrus, and that it is known in certain varieties of grapes and gooseberry. It is rare in apples, so far being found only in the Duke of Devonshire. He states that in pears there seems to be a strong tendency to variegation of stem and fruit and gives a list of twenty varieties in which striped forms have occurred. There is no mention in his article of variegated figs. Babcock and Clausen report that chimeras occur frequently in citrus fruits, fairly commonly in apples and pears, and have also been found in grapes, olives, and tomatoes.

In California individual trees of the Panachee fig have been located at the following places: Merced, Stockton, Davis, Fresno, Brentwood, Pasadena.

The origin of this sectorial chimera has not been learned. Barron describes it simply as a striped form of Col de Signora Bianca. That it may have originated as a sport of this well-known variety is a plausible explanation as the other cases described in this article show that fig sports are not uncommon. Since fig trees are propagated almost exclusively from woody cuttings this variegated sport has been easily perpetuated.

The distinctiveness and brilliancy of the bands of color upon the fruit are striking although there is some variation in their width. In some fruits the yellow color predominates and one might describe them, as does Hogg, as yellow with green bands. Others show the green color more prominently and these are described according to Eisen as "dark green, striped yellow." In a very few specimens the chlorophyll is entirely lacking but such figs seldom reach normal size or proper maturity. As the figs dry the green color fades and is imperceptible in the fully dried product. Except for their smaller size the dried figs are comparable to Calimyrna figs in appearance.

As mentioned by Roeding and as shown in the accompanying illustration, (frontispiece) the branches of the Panachee tree show variegation during the first year. The bands of color are seldom so distinct and vivid as on the fruit but are nevertheless very noticeable in contrast with the green foliage or with the gray of the older wood during winter. The leaves show no indication of variegation. Occasional branches occur in which the color is entirely green or nearly so both in stems and fruits. Seeds of the Panachee from caprified figs have been planted and their development will be watched with interest. The variety does not produce a crop the first year. The figs develop parthenocarpically but are improved in size and texture by caprification.

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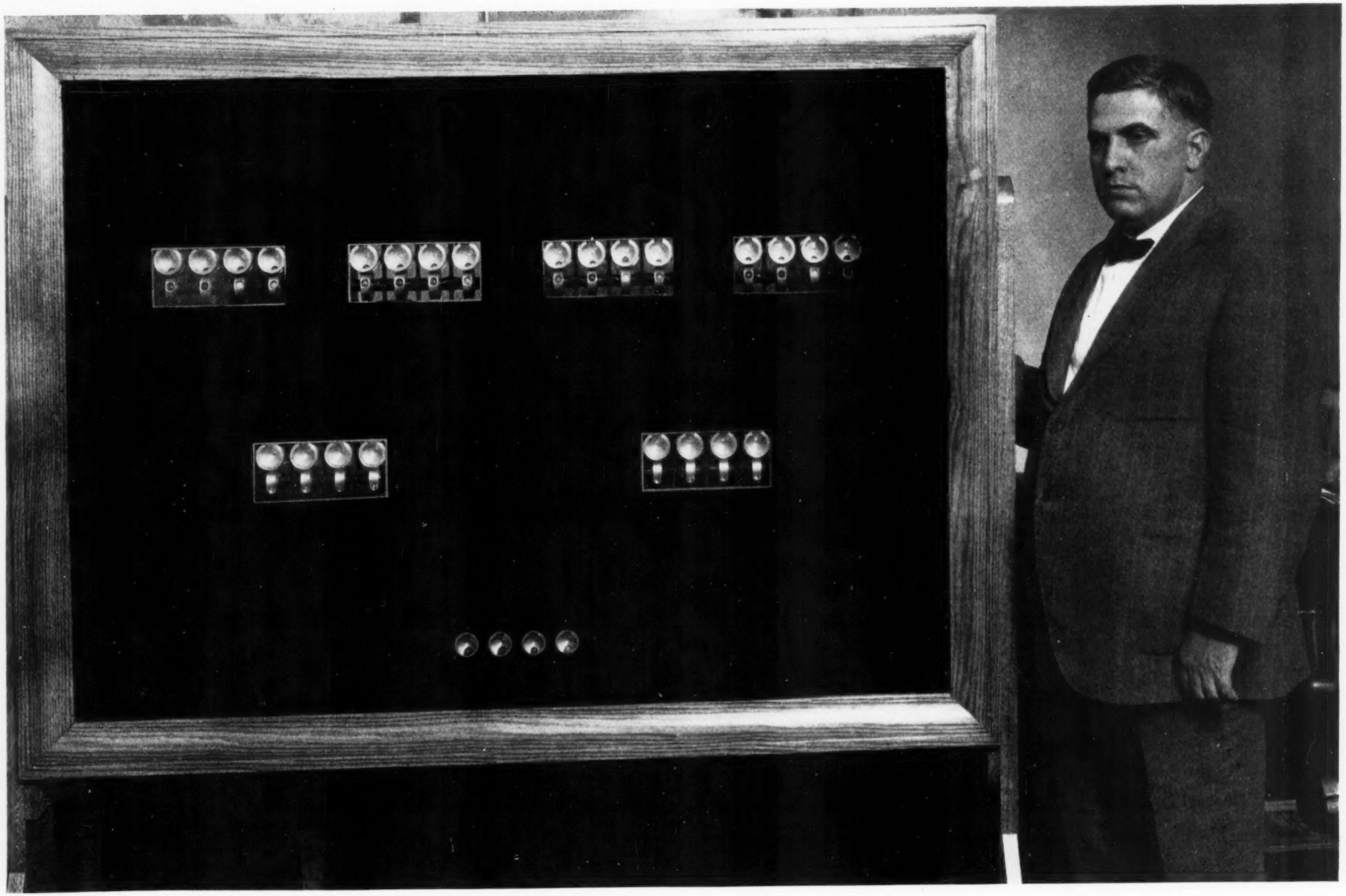
A Clergyman on Fundamentalism

"THE present noisy contingent, however numerous, of standpatter Fundamentalists, the men dogmatically intrenched in the ditches of exploded traditionalism, whose mental obsessions are so confirmed that they are 'never able to come to a knowledge of the truth'—this crowd will soon all be dead, and in a world era of growing enlightenment it can have no succession equal to its present mass. Its moblike and obstructive organization against intelligence cannot with equal audacity be reproduced in any future generation. In the long run, Ignorance always fights a losing battle against Intelligence. * * *

"While it is true that there are many older men in the ministry today that are alien, indifferent or unsympathetic toward the new scientific theories, either through ignorance or because they are non-progressively minded, I am impressed that of the younger members of the ministry, university trained, graduated from modern theological seminaries and still under 50 years of age, an overwhelming preponderance are evolutionists. Many of these, it is true, are not vociferous advocates of the philosophy, but both their belief and movement are in alignment with modern knowledge. It should go without saying that the influence of the intellectual morale of this great body of cultivated and influential religious teachers, distributed widely throughout the nation, should prove most effective in correcting and dissipating the blighting ignorance now so vocal in the land.

"As helpful to a sane confidence, we may remember that the present Fundamentalist fiasco is not the first historic exploitation of static traditionalism as opposed to scientific progress. But the statics have always finally been driven from the field. Truth, in its very nature, is invincible. Scientific truth, with a growingly harmonious and multiplying demonstration from a thousand seats of research and learning will stand triumphantly vindicated when the present falsely branded 'Fundamentalism' will be only a dim historic memory."—Rev George Preston Maines, "long prominent in affairs of the Methodist Episcopal Church and author of several well-known books on phases of religious thought," in *Current History,* March, 1927.



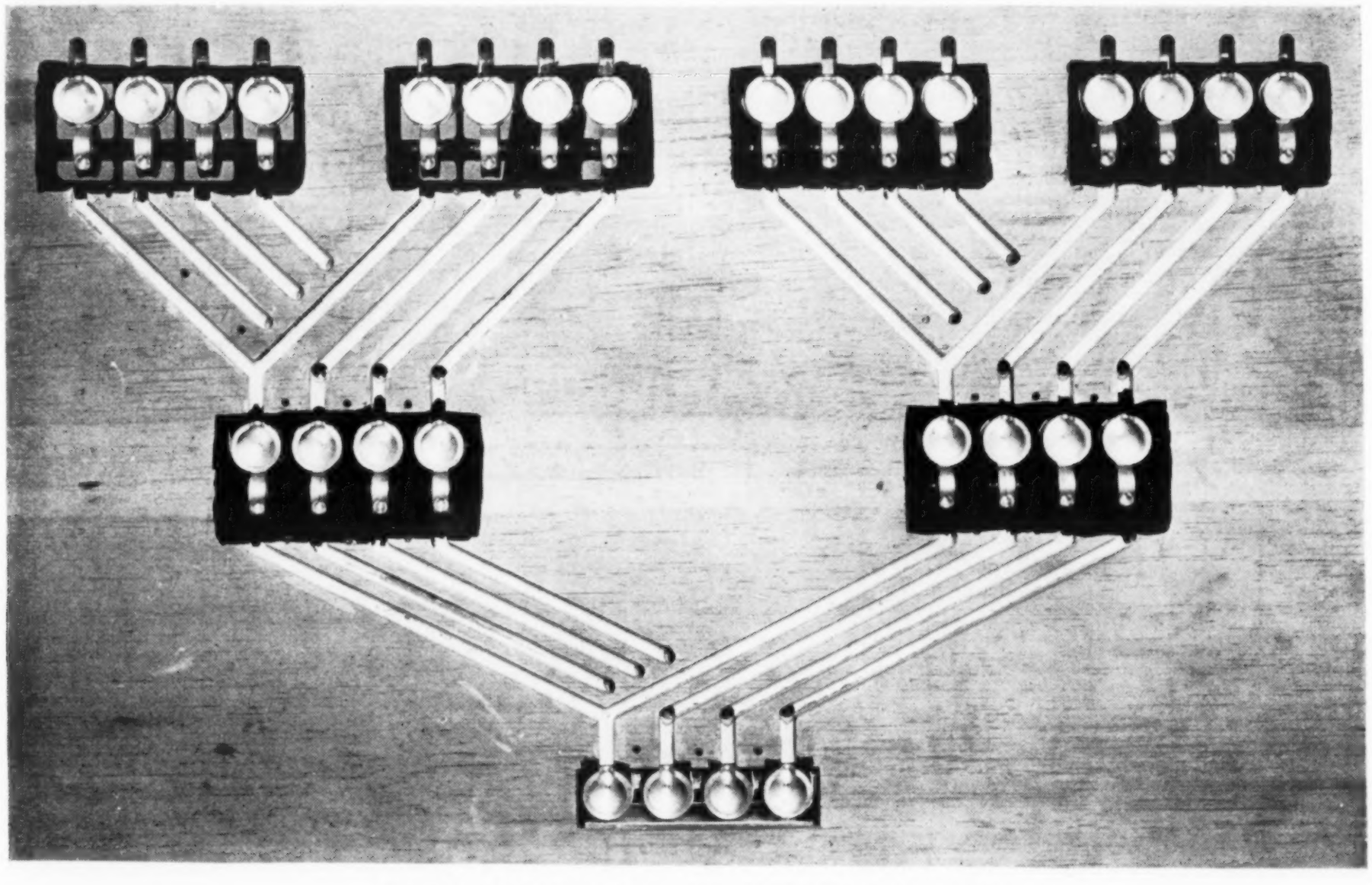
FRONT VIEW OF HEREDISCOPE

Figure 3

R. R. Graves, in charge of dairy cattle breeding investigations, Bureau of Dairy Industry, United States Department of Agriculture, with the "herediscope" which he has invented to illustrate the manner in which hereditary characters are transmitted from parent to offspring.

THE HEREDISCOPE

A New Contrivance for Demonstrating Mendelian Inheritance



CONSTRUCTION OF HEREDISCOPE

Figure 4

This view of the herediscope shows the connecting mechanism or channels by which the colored balls travel from one cup to the other. The apparently incomplete channels make their connections with the cups toward which they are headed by means of channel-bridges back of the channels they cross. It is evident that each cup in the "progeny" groups is connected by the channels with a cup in each of the two "parent" groups.

A MECHANICAL contrivance called a "herediscope" has been invented by R. R. Graves of the Bureau of Dairy Industry, United States Department of Agriculture, to aid in teaching the Mendelian theory of inheritance in dairy animals.

Inheritance, says Mr. Graves, is such a complex study that the average person is unwilling to expend the time and effort necessary to gain an understanding of the subject by reading about it. Furthermore, it is extremely difficult even for those well versed in the subject to write or lecture on heredity so that it can be understood by those who have not made some study of the subject. By means of this newly devised machine, tentatively named "herediscope," it is hoped the most simple and fundamental principles of heredity can be more easily explained to livestock breeders, county agricultural agents, extension workers, students of genetics, and others whose work demands that they have some knowledge of the laws of inheritance.

The use of the herediscope need not be confined to teaching inheritance in dairy cattle. Fundamental principles of heredity are the same in plants, animals, and humans.

The machine consists of a number of groups of aluminum cups, each group representing an individual ani-

charge of dairy cattle breeding which he has invented to illustrate the manner in which hereditary characters are transmitted. R. R. Graves, in charge of the "herediscope" with which he has invented to illustrate the manner in which hereditary characters are transmitted to offspring.

mal and arranged in the form of a pedigree showing three generations, four grandparents, two parents and one offspring. Numerous small, colored balls, each representing a given hereditary character, such as the "factors for high production of butterfat" or "low production of butterfat," are placed at random in the cups of the starting generation. When the operator presses a trigger connected with the mechanism, half of the character-symbols from each parent-cup are transmitted to the offspring, the selection of characters being merely by chance as is the case in actual matings. By repeated matings the observer is able to note how certain characters may be transmitted from generation to generation, or how they may be lost entirely in the process, how they may be present in an individual but covered up by dominant characters, and how in the absence of dominant characters they may again appear. The machine will illustrate the transmission and recombinations of four separate characters, or it will illustrate the transmission of four multiple factors, in such types of inheritance as milk yield, egg yield, stature or other quantitative characters.

Specific need for a better means of teaching the theory of inheritance was brought to the attention of Mr. Graves when as a result of his inheritance studies on the records made by dairy cattle of the various breed associations, the theory was advanced that the proved prepotent sire that was transmitting uniformly high production was one that was pure in his inheritance for the factors controlling high production, and that if such sires could be used for several generations, a strain of cattle would eventually be bred that would be pure in their inheritance for high production. It is not enough that a dairy sire be pure-bred or that his dam be a high producer. He must have an inheritance which is "pure for high production." In other words his germinal makeup must carry only those factors which cause high production; otherwise he would transmit low or average production to some of his daughters. Such sires whose germinal makeup is pure for high production are few and far between, but apparently they do exist. Unfortunately they can not be distinguished from sires whose germinal makeup is mixed, except by noting the production records of a large number of t h e i r daughters. When a sire is found to have a large percentage of his daughters in the high-production class, he is considered to be pure in his germinal makeup and is termed a "proved" sire.

Proved sires are being located by examining the records of cow-testing associations, bull associations, and individual herds. Such sires are retained for active service as long as possible. Until the average dairyman gains a better understanding of the laws of inheritance, however, he is not likely to realize the importance of the proved sire. One illustrated lesson with the herediscope should make it possible for him to see why the so-called proved sire is the most certain and speediest means of developing high production in dairy cattle.

As yet there is but one herediscope in existence, the one made by bureau engineers for Mr. Graves. Some of the mechanical features were designed by K. E. Parks and the machine was built by G. F. Betz. It may be possible to simplify the mechanism so that herediscopes can be produced without excessive cost, and small enough in size to permit their use in various places where county agents or extension workers find it necessary to talk on inheritance.

THE IMPROVEMENT OF CORN

By Selection and Plot-Inbreeding

T. B. Macaulay

THE development of cereal varieties that will mature in a short season and yet yield abundantly is of importance to all Canadian agriculturalists.

The writer is a Montreal business man who happens to own a farm on the Ottawa River, thirty-five miles west of the city. Eight years ago he became interested in the improvement of corn. The following is a brief account of the methods adopted and the results obtained.

The first question to be answered was whether any strains already existed that were suitable as grain producers in our northern climate. All promising varieties listed in the catalogues of leading seedsmen were tested. It was found that not one combined the essentials of earliness, yield, and reasonable fixity of type and maturity.

The next point to consider was whether it was within the limits of possibility to develop strains that in a short season would furnish yields approximating those of the later maturing varieties of the corn belt.

When the French explorers first reached the Island of Montreal they found, on the site where the city now stands, a stockaded Indian village surrounded by fields of corn. Attention was therefore directed to the local strains grown by some Quebec farmers. It was reasonable to suppose that this flint corn was descended from that grown in the same neighborhood by the Indians, centuries previously. Long motor trips were made through French-speaking sections, and a large number of more or less promising ears were thus accumulated. Through the kindness of Mr. F. D. Richey, of the United States Department of Agriculture, seed was obtained also of strains from Indian reservations. For the valuable advice and cordial cooperation of Mr. Richey at all times since then, we would express the deepest appreciation.

During the following two years many plots were sown, each receiving the seed of a single selected ear. The idea, at that time, was merely to find out which of the local farmers had the best strain, but as might have been foreseen, the results were very disappointing. The variations even among plants grown from the same ear were surprising and disconcerting. It was evident that the seed was, on the whole, of very mixed inheritance.

What was to be done? The problem was clearly much more complicated and difficult than had been supposed. There seemed to be no other course than to try self-fertilizing on a large scale. Next year some of the brightest young people from the adjoining village were employed to carry out the self-pollinating. When autumn came it was found, however, that every mistake and carelessness that could be imagined had been committed. There were multitudes of unfertilized ears, ears too completely fertilized to be genuine, white ears with yellow grains, and so on ad nauseam. The ears that were apparently selfed were usually nubbins. Whatever might be accomplished by this method in the hands of experts, in the hands of those on whom we had to depend the result was total failure.

Livestock Principles in Corn Breeding

This misfortune, however, proved to be a blessing in disguise. It compelled careful and independent reconsideration of the problem in all its

THE PLOT METHOD OF INBREEDING

Figure 5

Portion of inbreeding plot (some plants removed). The sheet separates the plot from the late maturing "barrier corn" in rear. At left, lane produced by leaving one row of hills unplanted. Soy beans growing with the corn. Note ears gathered but not yet husked. Each one of these isolated plots contains the progeny of a single ear.

aspects. The writer has been for many years a close student of the laws of heredity as they affect animal breeding, and the matter was naturally approached from that point of view. The Mendelian principle rules of course in the vegetable and animal worlds alike, and the problem gradually resolved itself into the question—To what extent can the methods of breeding which have done so much for the improvement of livestock be applied to the improvement of corn?

What are the principles which have guided the great creative breeders of livestock? Speaking broadly, they were two: rigorous selection, exercised concurrently with inbreeding, the object being to fix and, if possible, increase the good qualities possessed by their original animals. Inbreeding was considered of vital importance, but only as the handmaid of selection. Expressed in modern language these great breeders selected their foundation stock from herds or flocks with a minimum of mixed blood and therefore already having some desirable homozygous factors. They then endeavored, by selection and inbreeding, to transform the other desirable factors from a heterozygous to a homozygous state. If it were desired to add some new quality, or to accentuate some quality not sufficiently pronounced in the original strain, they did not hesitate to introduce fresh blood, but usually only through a half-outcrossed male, after which the selection and inbreeding were continued. If undesirable qualities appeared in any animal, that animal was promptly discarded. If all, or even most, of the inbred animals had shown signs of deteriorating, the entire strain would have been dropped.

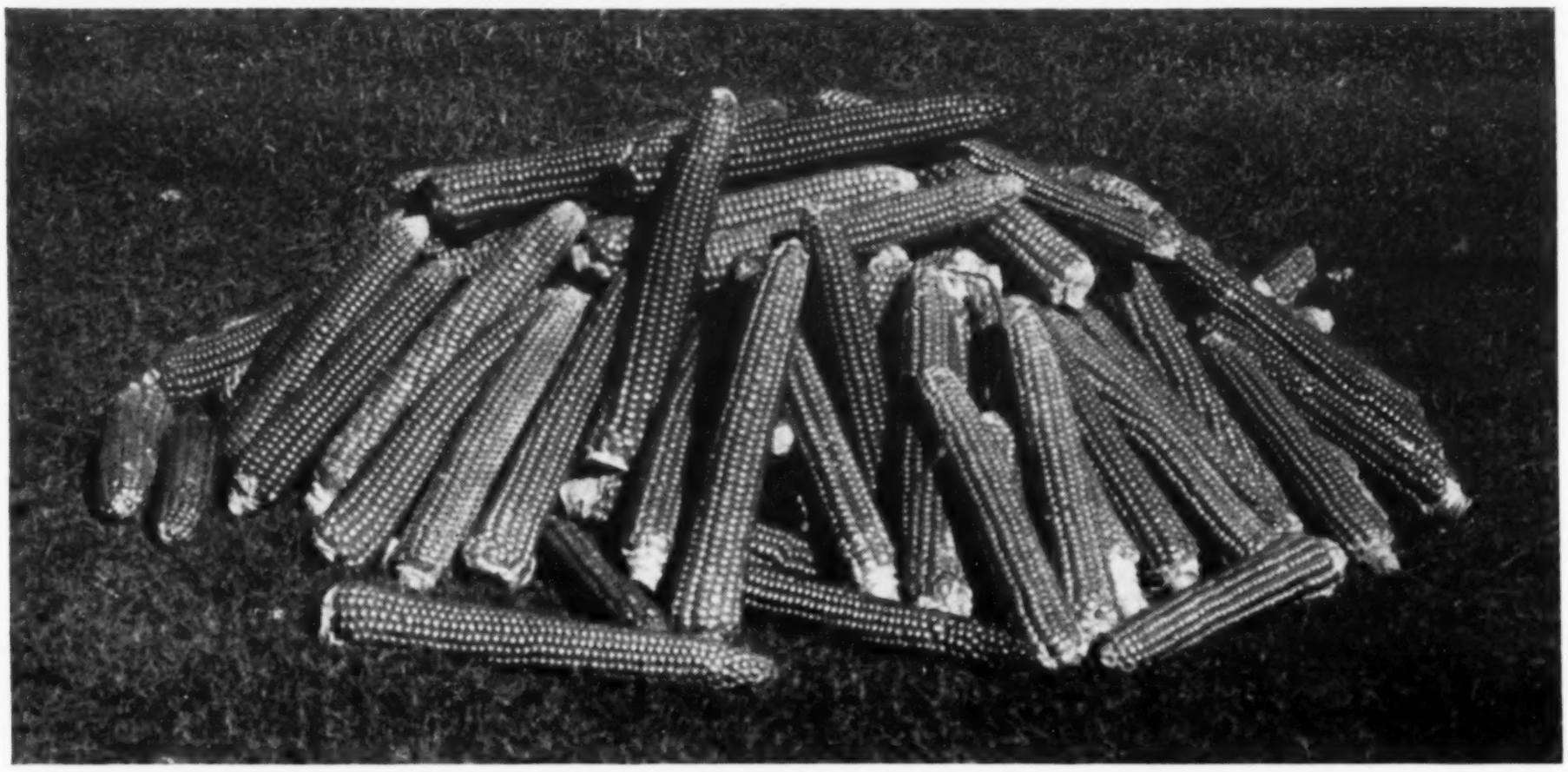
Could these livestock principles and methods be applied to the improvement of corn? No reason to the contrary could be found. Even though under our conditions matings could not be absolutely controlled as with livestock, yet they could be controlled within limits, and the numbers of the progeny and the rapidity of the generations would permit both selection and inbreeding to be carried out on a scale which would make possible the creation of superior strains more completely homozygous than any existing breed of domestic animals.

The objection, however, arose that self-fertilization in corn appears always to lower the vigor of the plant, and reduce the yield. But why should it not be at least theoretically possible to combine, in a homozygous condition, all the good qualities which, it had been proved, could, by judicious crossing, be combined in a heterozygous condition? It is true that this combination of a large number of factors that would be both favorable and homozygous could only be expected to occur once out of some huge number of possible combinations, but the thought arose that if the inbreeding were less abrupt than by self-fertilization, and if in the course of the inbreeding all individuals except those of superior quality were continously rejected, that rare combination might not be unattainable. With the acceptance of the concept that inbreeding in corn does not necessarily produce degeneracy, the whole outlook of the corn-breeder is altered. Instead of working in what has now become the usual way, merely for a temporary combination of desirable factors in a heterozygous condition, he begins to think out methods of securing combinations of at least a considerable number of such factors in a homozygous state.

Progressive Inbreeding By Plot Selection

The conclusion reached from these considerations was that progress was at least possible if a fairly large number of separate plots could be grown in isolated places, each plot sown with the seed of a single selected ear. If the plots were sufficiently far apart to prevent crossing, then every plant in a plot would be from seed

rear. At left, lane produced by leaving one row of hills unplanted. Each one of these isolated plots contains the progeny of a single ear. not yet husked.



EARS GATHERED FROM A TWELVE-ROW PLOT (C 5)
Figure 6

The produce of each plot is studied for desirable characters, uniformity, etc. If it measures up to standard a few of the best ears are used for planting in the individual plots the following year.

having the same mother ear, and even if in the previous year each individual grain on that ear had been fertilized by pollen from a different source, every plant growing in the plot would have a degree of relationship to the others at least as close as that of half-brother and half-sister. All seed produced on plants within such a plot, and necessarily fertilized by pollen from such near relatives, would, of course, be inbred. The process would correspond to the practice in animal breeding of mating one selected male with many half-sisters generation after generation, except that the line of descent would be carried on through selected female ears fertilized by many half-brothers, and also that an unknown but possibly considerable degree of self-fertilization would occur within the plot, thus making the inbreeding closer than in the livestock practice mentioned. If this process were continued for a sufficient number of years (probably not over four), the plants would become as closely inbred as if the original parent plant had been selfed. The same degree of homozygosity would be produced, but the work would be spread over a number of years instead of being consummated in one.

It was the fact that the process of inbreeding would be thus spread over several years that would give the method its value, and open the door of opportunity for the breeder. Think of the progeny of a single plant in even four generations. If each plant have one ear with 500 grains, the seed that could be produced from the single original grain would be in the fourth year $500 \times 500 \times 500 \times 500$, or 62,500,000,000, and in the tenth year the number would reach twenty-one figures and be unpronounceable. If every seed in this series could be sown in an isolated plot, then every one of this vast number would have factors that would be largely homozygous, and the opportunity for selection would be enormous.

Since, however, it is impossible to have millions of millions of isolated plots, it may be said that suggestions such as this have no practical value and lead nowhere. But is this so?

It is certain that of the ears pro-

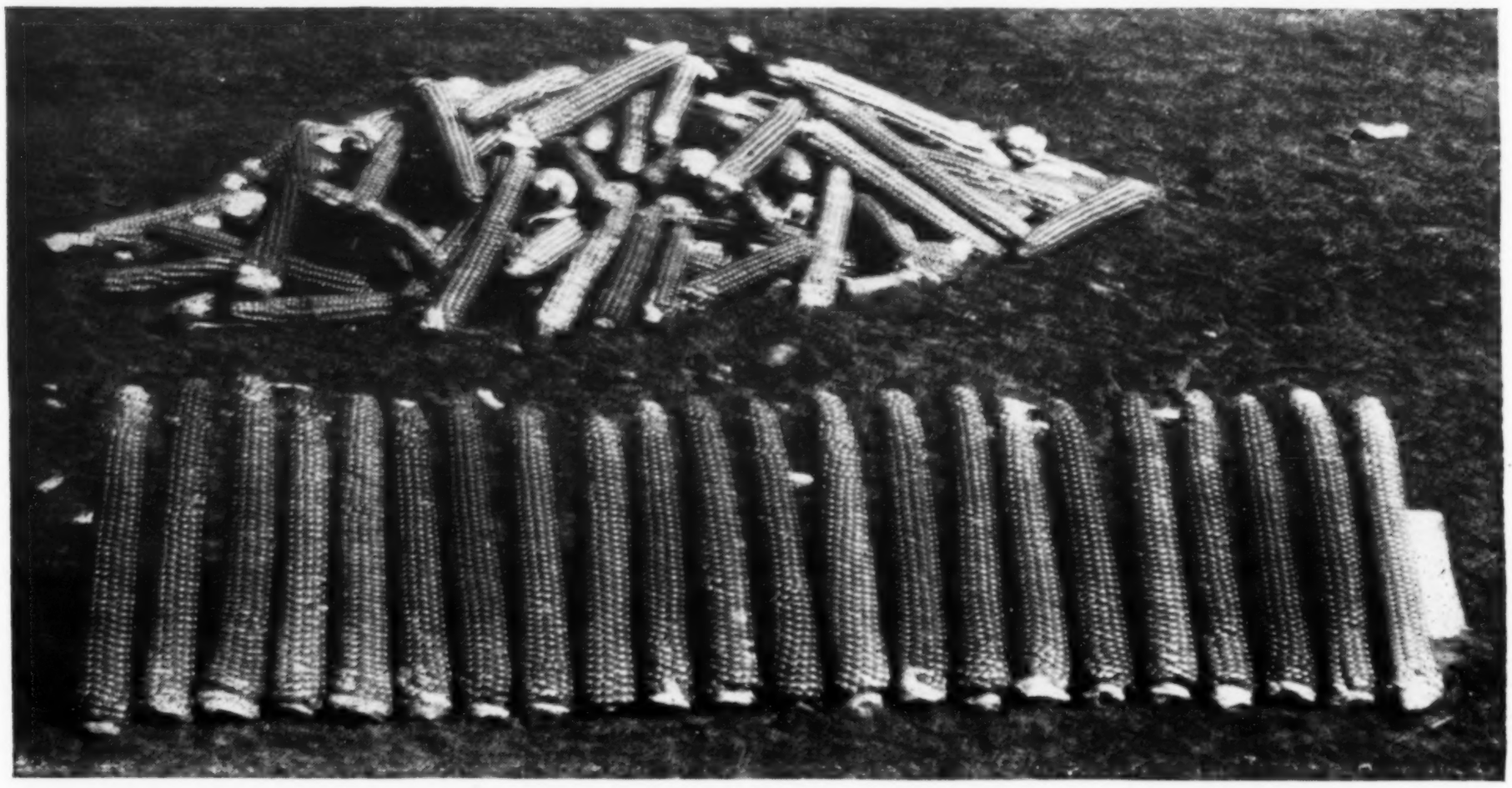
duced in the final plots of such a series but an exceedingly small percentage would be even equal to the average of the open-pollinated ears of the original strain, and a still smaller percentage superior. All the rest would be inferior, and they would be inferior because of the dropping out of desirable factors in the course of becoming increasingly inbred. Now if it be true that the vigor and heavy yield of cross-bred strains are due to most of the favorable factors being dominant, it follows that, when a desirable factor fails to show its effect in an inbred strain, it has been lost and can never be regained without an outcross to a plant from some other strain possessing the desired factor. Such degenerating strains would form an enormous percentage of all that would be produced under the supposititious complete system. For our purposes, every ear showing any material sign of degeneracy would be valueless and could be disregarded. By growing seed only from ears that were still of high quality, although becoming progressively inbred, all the lines of descent from which it would be possible for the ideal homozygous strain to develop could be retained. From a comparatively few ears all the desirable combinations could thus be obtained that could be secured by growing the entire number of possible ears in separate plots. This was a fact of vital importance in estimating the value of inbreeding by the plot method. If for practical reasons it should even become necessary to limit breeding to only the very choicest of the strains that were not degenerating, we could still know that we were retaining the best of the lines of inheritance from which alone our superior homozygous strains could possibly develop.

These considerations resulted in the adoption of a completely new program. The work of the previous years was not entirely lost. We now knew which were the most promising strains, and were able to concentrate on them, discarding by far the greater number of those with which we had begun. The existing strains moreover were already considerably inbred, although unfortunately there must also have been a certain amount of crossing because of the plots not having been sufficiently separated in those preliminary years.

We have now for four successive years had from one hundred to one hundred and seventy plots, each sown with the seed of a single selected ear. So far as possible these plots have been eight hills wide by eight to ten hills deep, and have contained from 200 to 250 plants. Our custom is to have hills three feet six inches apart and plots separated from each other on all sides by spaces usually about ten or twelve hills wide, these intervening hills being planted with a very late silage variety (Eureka) which does not tassel till long after the plants in the plots have finished silking. The space equivalent to a row of hills is left vacant on every side of each plot to avoid overshadowing by the higher barrier corn. It must not be supposed that, because this "barrier" corn is higher, it acts as an ideal obstacle to the spread of pollen from plot to plot. That was the original hope, but, in practice, it was found that the late dents at first grow more slowly than the flints in the plots and at tasseling time are not high enough to act is an effective barrier. Soy beans or any other crop would probably do almost as well for the separating belts. Corn, however, is still used for the "barriers" because it and the remnants of the plots can, at the close of the season, be harvested together and put into the silo.

In order to permit comparisons of relative earliness, plots are grown each year of two standard varieties —Quebec 28 and Howes Alberta. Comparisons based on dates would, of course, be unreliable because of the variations of climatic conditions.

During the summer, notes are made



"SELECTS" AND CULLS

Figure 7

Ears from another twelve-row plot (D 5). First draft of selected ears in front; culls in the rear. About five of the best ears from this plot will be used for seed next year.

from time to time on the condition of the various plots at the different dates.

Method of Selection

As the plants near maturity selection begins. When from thirty to forty per cent of the ears in anv plot appear ready, those ears that are ripe enough are at once gathered, all others being disregarded. The same course is followed with all the remaining plots as they near maturity. Our practice, in this connection, may not be without interest. The writer, accompanied by two farm assistants, moves from plot to plot, jotting in his notebook the average height of plants, uprightness, height of ears, degree of maturity and other features of each. He then sits on a camp stool and places on his knees a light board, three feet long, with a narrow cleat at each end. The assistants then gather the ripest ears. They are instructed to gather none that are not at their base at least as high as the knee. All ears from two-eared plants are kept separate. The assistants then begin husking and place the husked ears on the board. All that are undersized, off-type or immature are at once discarded. Notes are taken of the size, appearance and maturity of those saved. With newly husked ears slight differences in maturity stand out prominently, and distinctions can be made which it would be impossible to draw even a few hours later. The ears saved are then placed in a cheesecloth bag, tagged with plot number and date, and we move on.

As we proceed in this way we may come (though rarely in the last two years) to a plot that is fine, but green and late. It receives no other consideration than a note "too late." Its ears are not gathered, but pass at once into the discard.

The next plot may be early and ripe, but with rather inferior ears. It, too, is rejected entirely.

The following plot may have ears that are ripe enough but not of even size and type. Possibly one or two ears may be saved from this lot.

Then may come a plot that is early,

and also has many fine ears of satisfactory size and uniform type. Of these perhaps thirty or forty of the earliest and best are saved.

It will be noted that we do not weigh or measure the total production of each plot. We reason that, if we can develop plants that will produce uniformly large and satisfactory ears, the total yield per plot or per acre will look after itself, provided the danger of barren plants be avoided. To guard against this contingency every plot was this year carefully examined and it was found that there were practically no barren plants. Comparisons based on total yield per plot will hardly be necessary until the strains become more fixed in character.

The bags containing the saved ears are hung up indoors until thoroughly dry.

As convenience allows, the rough notes which have been made are posted in the record book under their respective plot numbers, and then during the winter months the ears on hand are again carefully scrutinized with a view to reducing them to approximately the number required to sow the plots of the following year. The measurements of each ear are recorded—the number of rows, the length of actual grain from stem to tip, the cirucmference near the butt, and its general characteristics. The final selections are made with the ears of all competing plots spread out in groups on a long table for comparison, and with the plot notes as an additional guide. Other things being equal, preference is given to those plots that show most uniformity. An effort is also made to reduce the number of original strains each year by discarding all those that are now represented by but few ears, unless those ears have special merit. By this severe culling the ears from about two-thirds of all the plots have, in the past, been each year completely discarded. Out of, perhaps, twenty-five or thirty thousand ears grown in all the plots, less than two hundred usually are saved for seed. The grain of these selected ears is then shelled and placed in small numbered cotton bags, with descriptive labels attached.

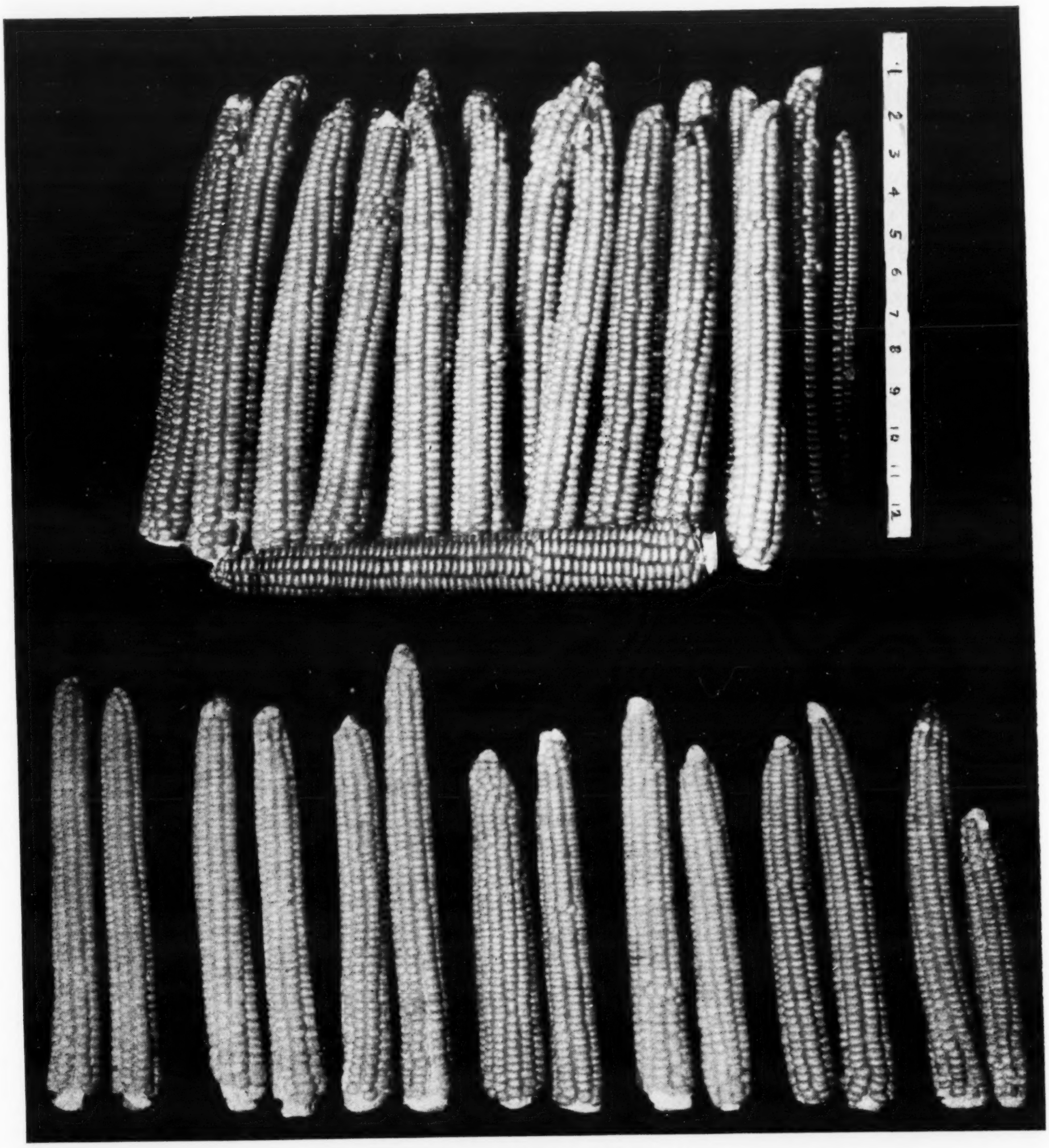
Effects of Natural Crossing

It will be objected, that with plots no further separated than ours, strains cannot be kept absolutely pure, and that a certain amount of crossing must occur. Quite true. We admit that we are not carrying out our theoretical plan as it should be carried out. If we had even fifty acres available each year we would certainly have a much larger number of plots, and those plots would be of sufficient size to use all the seed each selected ear could supply, and would also be separated by much wider barrier belts. The theory, however should not be condemned because of the deviations from the ideal condition which limitations of space have compelled us to adopt.

But to what extent does crossing actually occur? We can speak with some definiteness on this, as we have tested it by sowing plots of white corn alternately with yellow. The result was very reassuring. By far the greater number of the white ears were entirely free from any yellow cross. Other ears had some yellow grains, but usually very few. That there was some crossing was clear; but that the amount of such crossing was relatively small was equally clear. Our present strains are exclusively yellow flint.

As a further safeguard we group together all closely related plots, so that if there be any crossing it will come from a strain so nearly allied that little damage can result. The plots which we now have trace back through the maternal line to a surprisingly small number of the original ears.

Another point to note is that, under the system of plot inbreeding,



A TWO-EARED STRAIN

Figure 8

Ears from an eight-row plot (S 1). The pairs of ears on the right were each grown on one plant. This strain has been plot - inbred for six years. It is hoped that by a continuation of plot-inbreeding and selection for but a few years more, the production of two ears to a plant, and the other desired characters will be sufficiently fixed to be of immediate practical value. The photograph does not do justice to this strain as a number of the very best pairs of ears were inadvertently omitted.

a small amount of crossing is by no means as serious as if the plants were being self-fertilized. The foreign blood thus introduced is inevitably swamped in the following year by the overwhelming amount of pollen from the plants in the plot which grew from grains of the ear that were not crossed, and this ensures that there will be but little disturbance in the course of the inbreeding.

But it may be said that, if a grain in one of the selected ears should happen to have been crossed, it would be likely to produce a superior ear which would be chosen as seed for the next generation. Possibly this may be true. But if we have been able to maintain in the inbred strains those dominant factors whose presence in crossed strains causes hybrid vigor, an ear from a crossed grain will not necessairly show superiority.

But let us suppose that the cross-fertilizing of an occasional grain should produce, in the next generation, a superior plant and superior ear, what would that mean? Merely that the chance cross had restored some favorable factor or factors which had been lost in the inbreeding, or that it had added some such factor or factors not previously possessed by the strain. That may interfere with the theoretical purity of the strain, but let us view the matter not with the eyes of the theorist but with those of the practical breeder. Would not any of the great live-stock improvers have welcomed a partial outcross that would work improvement in his strain? It is true that he would then have continued his inbreeding to his original strain, so that the added influence would have been reduced to the relationship between a small tributary and the great river into which it flows. That, however, is precisely what will usually happen in the case of a chance crossing coming to an inbred plot. Its effect would be exactly what an animal breeder would desire. It is possible therefore that these occasional outcrossings may sometimes have been blessings in disguise.

In any case, whatever we may think as to the relative purity of these plot strains, that they are intensely inbred as compared with open-pollinated corn cannot, we think, be denied.

What Have Been the Actual Results?

It must be confessed that we awaited the harvests of the first two inbreedings with some anxiety. That the deterioration noticed by other experimenters would be in evidence was to be expected. The question was, to what extent? Would there be many ears equal in size and quality to the choice ones the seed of which had been sown?

The results reassured us. At first we had little hope beyond that of developing a few strains equal to the original ears, but, because of their inbreeding, more uniform and prepotent. That this could be done we were soon convinced, though the amount of discarding that was necessary was somewhat discouraging. But as the process of selection went on year after year, and the inbreeding became more intense, not only did the plots become more uniform within themselves in type and maturity, but most of them also became earlier, and we noted with special satisfaction that the best ears in many plots were even larger and finer than their parent ears. This tendency has become steadily more marked. We have throughout, acted on the assumption that in early maturing strains increased yield can be obtained more readily by greater length of ear than by greater number of rows or by greater circumference. We do not ignore these other features and neither do we ignore vigor, uprightness, etc., but in selecting for yield, we have concentrated chiefly on length of ear, measuring from the beginning of the grain at the butt to the end of the last

TABLE I—Representative Records of Plot Inbreeding Experiments. Each Line Gives the Dimensions of a Single Ear, which is the Parent of the Ear Described Below It

Plot L.2 (1927) L. 2 10 - 10-1/4 x 5 x 8 - 10-1/2 x 5-1/2 8 - 10-1/2 x 4-5/16 8 - 10-7/8 x 5 8 - 11-3/8 x 4-7/8 8 - 11-3/4 x 5-3/8	L.3 12 - 9-3/4 x 6-1/4 x 12 - 9-1/4 x 5-1/8 12 - 10-1/2 x 6-1/4 12 - 11 x 6 12 - 11-5/8 x 5-3/4 12 - 11-3/4 x 5-3/4 12 - 9-1/2 x 5-1/2 } xxxx 12 - 9-1/2 x 5-5/8 }	L.1 10 - 10-1/4 x 5 x 8 - 10-1/2 x 5-1/2 8 - 10-1/2 x 4-5/16 8 - 10-7/8 x 5 8 - 11-3/4 x 4-3/4 8 - 12 x 4-7/8	-2- M.1 8 - 7-1/4 x 5-1/4 x 8 - 10 x 5 3 - 9-1/4 x 4-3/4 8 - 10 x 5-1/8 8 - 10-7/8 x 6	M.2 8 - 7-1/4 x 5-1/4 x 8 - 9-3/8 x 4-3/4 8 - 9-1/2 x 4-3/8 8 -10-1/2 x 5-1/2 8 -10-3/4 x 4-3/4 8 -10-1/2 x 5 8 -11-3/4 x 5-1/4	P.2 12 - 10 x 5-3/4 xx 12 - 11-5/8 x 5-7/8 12 - 10-5/8 x 5-1/4 14-12 - 11-3/4 x 6-5/8 16 - 12 x 6-5/8	P.3 12 - 10, x 5-3/4 xx 12 - 11-5/8 x 5-7/8 12 - 10-5/8 x 5-1/4 12 - 11-1/4 x 5-5/8 12 - 12-1/3 x 6	P.1 8 - 7-1/8 x 4-3/4 xxx 10-8 - 8-1/4 x 4-1/2 8 - 9-1/4 x 4-1/2 8 - 10-1/4 x 4-1/2
						P.3 8 - 7-1/8 x 4-3/4 xxx 10-8 - 8-1/4 x 4-1/2 10-8 - 9-3/8 x 5 10-8 - 11-1/8 x 5-1/2	B.1 12 - 10-1/4 x 5-5/8 xxx 12 - 10-3/8 x 5-3/4 12 - 11-1/4 x 5-3/4	C.1 12 - 9-1/2 x 5-3/4 xxx 12 - 10-1/2 x 5-1/2 12 - 11-5/8 x 5-5/8
I. 4 12 - 8-3/4 x 5-7/8 x 12 - 10-1/4 x 5-3/8 12 - 11 x 6-1/8 12 - 9-1/8 x 5-1/8 } xxxx 14 - 5-3/4 x 5-1/8 } 12 - 11-3/4 x 5-5/8 12 - 10-1/2 x 5-5/8 12 - 11-3/4 x 5-1/2 12 - 10 x 5-1/4 } xxxx 8 - 8-3/4 x 4-1/2 }	J. 2 12 - 8-3/4 x 5-3/4 x 12 - 11-3/4 x 5-3/4 10 - 11-3/4 x 5-7/8 12 - 10-1/2 x 5-1/2 12 - 11 x 5-1/2 14-12 - 11-1/8 x 6 14-12 - 11-1/2 x 6-3/8	S.1 8 - 11-3/8 x 5-1/2 x 8 - 11 x 4-3/8 8 - 12-1/8 x 5 8 - 9-1/2 x 4-3/4 8 - 10-3/8 x 5 8 - 10-1/8 x 4-1/2 } xxxx 8 - 10 x 4-1/8 } 8 - 12-1/4 x 4-3/4 also 8 - 10-3/4 x 4-7/8 } xxxx 8 - 8-1/2 x 4 }	T.1 8 - 11-3/8 x 5-1/2 x 8 - 11 x 4-3/8 8 - 10-1/8 x 4-3/4 8 - 10-1/4 x 4-1/2 8 - 10-1/2 x 4-3/4 8 - 12 x 5 8 - 10-3/4 x 5-1/2 } xxxx 8 - 10 x 4-7/8 }	T.4 8 - 9-1/2 x 4-5/8 x 8 - 11-1/4 x 4-3/4 8 - 10-3/8 x 4-3/4 8 - 10-3/4 x 5-1/8 8 - 10-3/8 x 4-5/8 8 - 10-1/2 x 5-1/4 8 - 10-5/8 x 5-1/4 8 - 11-3/8 x 5-1/2	C.4 12 - 10 x 5-1/2 xxx 12 - 10-1/8 x 5-7/8 12 - 11-7/8 x 6 D.5 12 - 9-1/2 x 5-3/4 xxx 12 - 10-7/8 x 6-1/8 12 - 11-1/4 x 6	C.5 12 - 10 x 5-1/2 xxx 14-12- 11 x 5-1/2 12 - 11-3/4 x 5-1/8	D.4 12 - 10-1/4 x 6-1/4 xxx 12 - 10-3/8 x 6-1/2 12 - 10-3/4 x 5-7/8

x Original Ear
xx F_1 Hybrid between two strains
xxx F_1 Hybrid, result of crossing two strains and crossing again to one parent strain.
xxxx Two ears on one plant.

developed grain at the tip. In the early days many of the ears used for seed were but nine inches and a fraction in length of grain; for the ears used in 1926 we were able practically to place a minimum of ten inches, except in the case of a few extra early strains. In 1927, however, we were able to raise the minimum, and the length of grain in a large proportion of those sown was in excess of eleven inches. Of those saved from the 1927 crop for 1928 seed, the average is still greater. The crop of last season has in fact been a remarkable one. In a year which has been notoriously unfavorable, our strains have shown marked improvement. Uniformity of type within the plots has been increasingly noticeable. The average quality of the ears from the entire field has been decidedly higher. Until very recently the ears from nearly two-thirds of all the plots in the field would be discarded entirely because of immaturity or inferior quality. This year the proportion to be so discarded is relatively small, and the average of all is so much higher that selection is becoming increasingly difficult. A large proportion of the culls of this year are as good as the selects of two or three years ago. The movement toward improvement has been gaining momentum. There are so many promising ears that it is not easy to choose between them. It is evident that selection on a wider base is now desirable and arrangements are therefore being made to extend the scope of the experiments next year by planting a larger number of plots.

The following comparisons illustrate the progress which has been made with some of the strains. (See Table I). The symbols used are to be interpreted as follows:

8—10½ x 5 means an ear with 8 rows, with 10½ inches of length of grain measured along the cob, and 5 inches of circumference about 1 inch from the butt. Each ear described is the parent of the one below it.

The irregularities seen in some of the records were to be expected since in the initial years the plants growing within any plot are far from being of uniform inheritance and the crossing which takes place even within the plot must therefore produce considerable variation, which however rapidly diminishes as the inbreeding proceeds.

A Two-Eared Strain

A noteworthy feature of the 1927 crop has been the number of plants having two ears. Secondary ears of this kind are usually so inferior as to be of little value, but this year in a considerable proportion of these cases the secondary ears have been almost equal to the leaders in size, type and maturity. There must be a limit to the length of individual ears, and it may be doubted if much can advantageously be added to ears measuring twelve inches of grain. But we have had quite a number of cases where two ears grown on one stalk have unitedly measured more than twenty inches of solid grain. This characteristic has been very noticeable in one long eight-row strain. It opens further alluring possibilities, and these choice two-eared strains will hereafter be given special consideration.

As the result of our experience we now claim with confidence:

(a) That corn can be inbred, even to an extreme degree, without loss of vigor or impairment of yield, provided the inbreeding be carried out gradually in the manner outlined, and provided sufficiently large plots and a sufficient number of such plots be grown to permit of selection on a large scale, and provided also that the selection be drastic and continuous.

(b) That under this system inbred strains can be developed that will be even superior to the original open-

METHOD OF STORING SELECTED EARS

Figure 9

Some cheese-cloth bags hung on improvised rack to illustrate practice. Each bag contains the very choicest ears of one plot, and is hung up in the house until those ears are thoroughly dry. Unselected ears from a twelve-row plot beneath; also field note-book and record book. Ottawa River in the distance.

pollinated ears from which they are descended.

Plot Inbreeding vs. Selfing

Since it is by plot-inbreeding that these results are made possible, it is desirable to compare that method with self-fertilization in greater detail.

Under the plot method every ear is open-pollinated, *within the limits of the closely related plants comprising the plot.* This facilitates the formation of every possible combination within those limits, and thus permits very extensive selection, accompanied by a steadily-increasing degree of consanguinity.

By self-fertilizing approximately fifty per cent of all heterozygous factors are reduced to homozygosity in one generation. By plot-inbreeding it may take several years to attain an equal degree of genetic purity. But contrast the number of strains produced by the two systems in the course of reaching that condition. As previously pointed out a single plant can produce 62,500,000,000 grains at the end of the fourth year and this is the number of strains theoretically possible under plot-inbreeding. As already explained, it is, however, not necessary to produce any such number of strains, since an improved homozygous strain can only come from plants that, as they become inbred, show no sign of degeneracy. By maintaining for the four years even 500 inbreeding plots, each of sufficient size to accommodate all the seed on one ear, it is possible to retain at least a very large proportion of all the most promising lines of descent out of the whole 62,500,000,000 potential strains. Let us be very moderate and assume merely that these plots would contain the most promising lines out of the odd 500,000,000 strains. Opinions may differ as to this figure. Some of us will think it much too small. But whatever the number may be which will be thought reasonable, contrast it with 500 grains or possible strains on the single ear, which would be the entire progeny of the original plant during the one year that alone would be required to produce an equal degree of homozygosity in the seed by the system of self-fertilization. In actual practice such a self-pollinated ear would be very unlikely to be fully fertilized and the number of grains produced would probably be much less than 500. Is it not evident that the possibilities for selection under plot-inbreeding exceed the corresponding possibilities under self-fertilization to an extent almost inconceivable?

But there is another difference that that is even more important. Not only are the strains which may be isolated by self-fertilization painfully restricted in number, but they are also, of necessity, inferior in quality. Strains of the highest value are automatically excluded from the self-fertilized group. Plants grown from a self-fertilized ear can only inherit such favorable factors as were possessed by the parent ear. Crossing, such as occurs within the limits of a plot, is prevented. But, in an open-pollinated variety, the chance of finding one plant which will contain all, or even a large proportion of, the favorable factors is so small that it may be ignored. No one plant therefore can pass on all such favorable factors to its self-fertilized progeny. Additional generations of self-fertilizing will accentuate this condition. By the plot system on the other hand it is at least possible gradually to combine into a single homozygous strain all the favorable factors possessed by any of the combined plants constituting the plot. It follows, therefore, that besides being very restricted in number, the factor combinations that can be produced by continuous self-fertilization do not have the possibilities of those produced by plot-inbreeding. The inferior quality of most self-fertilized strains is, in fact, their most striking characteristic.

Although it may take a few years of the plot system to produce the

same degree of homozygosity as is produced in one year by self- pollination, the results reached are not at all identical. In the one case much of the genetic fixity will consist in the loss of favorable factors and the establishment instead of undesirable recessives in a homozygous condition, while in the other case the factors rendered homozygous should be chiefly, perhaps entirely, the desired favorable dominants. A similar combination of desirable factors in a homozygous condition could only be attained by the road of self-fertilization by reassembling the favorable factors by crossing a sufficient number of the inbred lines, and then beginning anew the course of self-fertilization and selection, and repeating this operation as often as might be necessary. Each such crossing would of course impair the homozygosity previously built up.

It of course cannot be denied that desirable factors may be lost in the course of inbreeding even by the plot system. It would be unreasonable optimism to think otherwise. It is quite certain, however, that many, possibly most of them, can be retained. Every addition to the number and size of the plots increases the number of factor-combinations that will be made, increases the probability of including in one plant the maximum number of desired factors, and increases the measure of success which may fairly be expected.

To ascertain whether any favorable factors are thus dropping out, and to remedy that condition should it arise, it may be desirable every few years to add a special plot sown with mixed seed obtained from the choicest ears of closely-related strains and thereafter to continue to select within the limits of that plot in the usual way.

But let us now pass to a further development of the problem.

Outcrossing and Backcrossing

Even though remarkable results can be obtained from plot-inbreeding as described, it does not follow that even better results may not be obtained by the grafting on to that system of a little judicious outcrossing. By doing this we may add some desirable factor or factors not possessed at all by the strain.

Let us, for example, consider any of the very early but very small varieties which are at present found near the far northern limit of the area in which corn ripens at all. These existing northern varieties have the essential quality of earliness, but how is it possible to obtain large size and heavy yield by merely applying selection and inbreeding to ears so small that five and a half inches of length is large? We have tried it and failed. We have, moreover, crossed several of these extra-early strains in the hope of obtaining satisfactory size through "hybrid vigor" but have again failed to secure anything worth while. The factors for size appear to have been lost and no amount of selection or of crossing with other strains also lacking those factors can restore them. When, however, blood from a large but later variety has been introduced the response has been immediate and marked, and both size and earliness have been combined to an extent that is very promising.

Or let us take the case of a strain which has excellent size, but is too late to be ideal. By plot-inbreeding and selecting for earliness as well as for size we have proved that it is possible to make such a strain earlier and earlier while yet retaining the size. The process can, however, be accentuated and speeded up by introducing an outcross of a specially early strain.

Our experiments have emphasized the desirability of choosing as foundation stock some variety that is already more or less uniform in type. By doing this, much discouraging work, otherwise necessary, may be avoided for many desirable characters will already be in a homozygous condition. This may seem a mere platitude but its truth has been brought home by the greater difficulties encountered in

dealing with ears of such mixed blood as some of our originals evidently were. The more mixed the inheritance the more difficult is it to isolate desirable strains. More and larger plots are necessary since a larger proportion of all ears grown will have to be discarded.

Our experience with absolutely cross-bred plots has also not been particularly encouraging. This is not surprising. In animal breeding a cross between two breeds may produce superior individuals, but to create a new and fixed strain from such cross-bred animals would be difficult. The same is true of corn. On the other hand, if such a cross-bred strain be recrossed to one of the parent strains the result may be surprisingly good. Two of our most promising strains were created in this way. In one case, an inbred strain of Quebec 28 (a fairly early, twelve-row yellow flint of medium size) and Triumph (a rather late twelve-row yellow flint of large size and fine type) were crossed, and the hybrid recrossed with the inbred Quebec 28. By continued selection, a strain has been developed equal in size and type to Triumph, but much earlier. In another case, Howes Alberta (a very early eight-row yellow flint of even type but small size) was crossed with a large local flint and the hybrid recrossed with Howes Alberta. In this case ears have been produced almost as early as Howes, but much larger.

When we consider the Mendelian aspects of this question, the advantage of this second cross is evident. By the first cross all factors not held in common by both parents would be thrown into a heterozygous state. How far-reaching this disturbance may be we cannot tell, but to evolve from these cross-bred elements a fresh set of satisfactory and mutually consistent homozygous factors, may not be easy. Inbreeding will cause segregation of factors, half towards the type of one parent, half towards that of the other. But by giving a second cross of one of the parental strains (if homozygous) one half of all the disturbed factors at once revert to a homozygous state and all such reversions are to the type of the one parent. It thus becomes possible by selection to graft on any desired quality from the other strain with a minimum of disturbance to the inbred line. The practice is in exact accord with the principles of enlightened live-stock breeding, for one of those principles is that a full outcross should never be introduced but merely a half outcross.

Although complete homozygosity is yet far off, we have, we think, gone far enough to indicate that the method we have adopted is entirely practicable and that by its use the problem of corn improvement becomes comparatively easy of solution. An outstanding feature of the system is that it can be carried out even on a large scale with a minimum of labor. Although our own work has been confined to the development specially required by ourselves, the principles and methods set forth are equally applicable to the development in a homozygous state of qualities of any description.

So much attention has been drawn to the increased yields that can be secured by crossing inbred strains that there is danger of losing sight of the fact that the production of hybrid seed of even the most productive quality has a temporary value only and that the real goal in corn-breeding must be the establishment of strains that will have all the good factors of the best F_1 hybrids but in a homozygous state. The plot system supplies a method by which this goal may be reached, and incidentally it should be noted that the improvement secured by it is visible from year to year, and the strains in process of development may have a practical agricultural value long before they become completely homozygous.

Under the plot system there stretches before the corn-breeder a vast and

never-ending field of possibilities. There seems no limit to what may be accomplished. Not only can any foundation strain be improved and fixed, but by combining related strains still further improvement can be secured, and by partial outcrossing in the manner suggested, still other desirable qualities can be introduced. And as these strains will be in a relatively homozygous condition, they will have the great advantage of degenerating but little, even if selection be discontinued. Truly the vision of future possibilities reaches to the horizon.

Social Science Abstracts

THE Social Science Research Council announces plans to establish a Journal of Abstracts in the Social Sciences. These plans are the result of five years study by a committee of the Council which has canvassed the situation with respect to the needs, resources, and purposes to be served by a comprehensive abstract service in the social sciences.

A substantial subsidy has been provided for a period of ten years until the journal has become self-supporting through subscriptions.

To assist the Organizing Committee, a number of advisory committees have been appointed in the fields of cultural anthropology, economics, history, human geography, political science, sociology, and statistics. These advisory committees have been asked to suggest: (1) the names of scholars who may be considered for the position of salaried editors and unsalaried consulting editors; (2) to draw up a scheme of classification adequate to the needs of the systematic grouping of materials from the respective fields of specialization within the social sciences.

Since the Council is made up of delegates from the national learned societies in the fields of anthropology, economics, history, political science, psychology, sociology, and statistics, the purposes of the Council in its efforts to further cooperative scientific research in the social sciences is best served by devoting Social Science Abstracts to the field of cultural anthropology, history, human geography, political science, sociology, and statistics, broadly construed.

Social Science Abstracts will be issued monthly during the year and in each issue will appear systematic abstracts of new information published in the fields indicated for the preceding month or months. Social Science Abstracts will be printed in English in this country, but it will attempt to cover the social science literature of the world as originally published in all languages.

E. Stuart Chapin,
Chairman of the Organizing Committee,
University of Minnesota.

EUGENIC STERILIZATION IN CALIFORNIA

7. Fecundity of the Insane*

PAUL POPENOE, *Pasadena, California*

THE facts that (1) at least one-half of all those admitted to hospitals for mental diseases show a history of mental disease or defect among their relatives[1] and (2) forty per cent of the children of the insane themselves become insane[2], indicate the importance of thorough study of the fecundity of the insane, in any program of mental hygiene or eugenics.

This feature was therefore included in a comprehensive survey[3] of the working of the California sterilization law, which has been under way two years, financed and directed by E. S. Gosney of Pasadena in consultation with an advisory committee made up of recognized experts in many different lines.

Table I gives the number of living children, by age at operation, for both males and females, and these data are represented graphically in Figure 10. Only the married are included in these statistics. The mean size of family among the married men is 1.64 ± .09 children, among the married women 2.03 ± .03. If all the sterilized patients, single and married alike, are included in the calculations, the mean family is reduced to 0.61 ± .26 children per man and 1.85 ± .03 children per woman.

Not Self-Perpetuating

It is a generally accepted fact among eugenists that a family of three children is not sufficient to keep a population from declining in numbers, while a family of four children will cause a slight increase in the population. It appears from the table and graph that the males are never fecund enough to reproduce their own numbers but that the completed family of the females is just about large enough to maintain the group intact from generation to generation.

In order to throw more light on this point, and to make the picture as simple as possible, we have fitted a straight line, by the method of least squares, to the graph for women in Figure 10. From a simple inspection of this, it is evident that the fecundity of the married, insane woman of the type who is sterilized can be reckoned conveniently as one child for every 15 years of her age.

Every year's delay, after puberty, in sterilizing her, means the addition to the population of .06 or three-fiftieths of a living child.

While this graph is based on the married insane, it will doubtless apply to those who are going to marry. It will not apply to the whole female insane population for, as pointed out in the preceding paper of this series, a large part of the insane never marry.

These figures for the fertility of the insane are maxima instead of minima (so far as California is concerned) because there has been in many cases a selection of the insane for sterilization, those that already had as many children as they could care for being sterilized. This is shown clearly by comparison with our controls, of which we took 100 of each sex from the files of a Southern California hospital, and a like number from one in Northern California, 400 in all. Table II presents these facts, showing that in either sex the mean number of living children per marriage is not more than 1½.

Size of Completed Family

To verify the size of completed families among the insane, we have tabulated the families of married women over 40, among the controls,

*A list of the other papers published in this series will be sent on application to E. S. Gosney, 26 N. Marengo Ave., Pasadena, California.

and present them in Table III. The mean number of living children is 1.74 among these women who have never been sterilized but who, because of their age, are almost certain to have no further pregnancies. From another point of view, only 17 per cent, or one-sixth of these women, had enough children to increase the size of the group to which they belong.

We have also tabulated the number of total pregnancies, including not only the living and dead children, but still-births and both spontaneous and induced abortions, so far as shown in the record. Certainly these figures are not complete, but they help to fix a maximum for the fecundity of insane women, giving some idea of what the size of the family might be, under the most favorable circumstances possible, that is, if every conception resulted in a living child. Even on this basis, the mean number of pregnancies is only 2.83, which is not enough to keep the group from declining in numbers.

On all grounds, then, we conclude, that in general a group of insane women, of the type that finds its way into California hospitals, will not wholly reproduce itself even if it is not sterilized. Still less will the men do so.

Differences Among Diagnoses

Table IV classifies all the sterilized insane in six groups, according to diagnosis, giving the mean number of total pregnancies for each, together with the mean age at time of operation. Table V similarly shows the number of living children of married women (including 31 single women who bore illegitimate children), by psychosis, the ages being the same as those in Table IV; with the controls for comparison.

When the probable errors are taken into consideration, we are obliged to consider that there are really no significant differences between the sizes of family (living children) of women having the various psychoses, in the group that has been sterilized. This is partly because the more fecund in all groups have been selected for sterilization. The variation is probably a little greater among the unsterilized. There is, however, a significant difference in the number of pregnancies of women with maniac depressive psychoses, as compared with most of the other groups. Those women have borne more children, but have also lost more children, than the average.

Religion and Fecundity

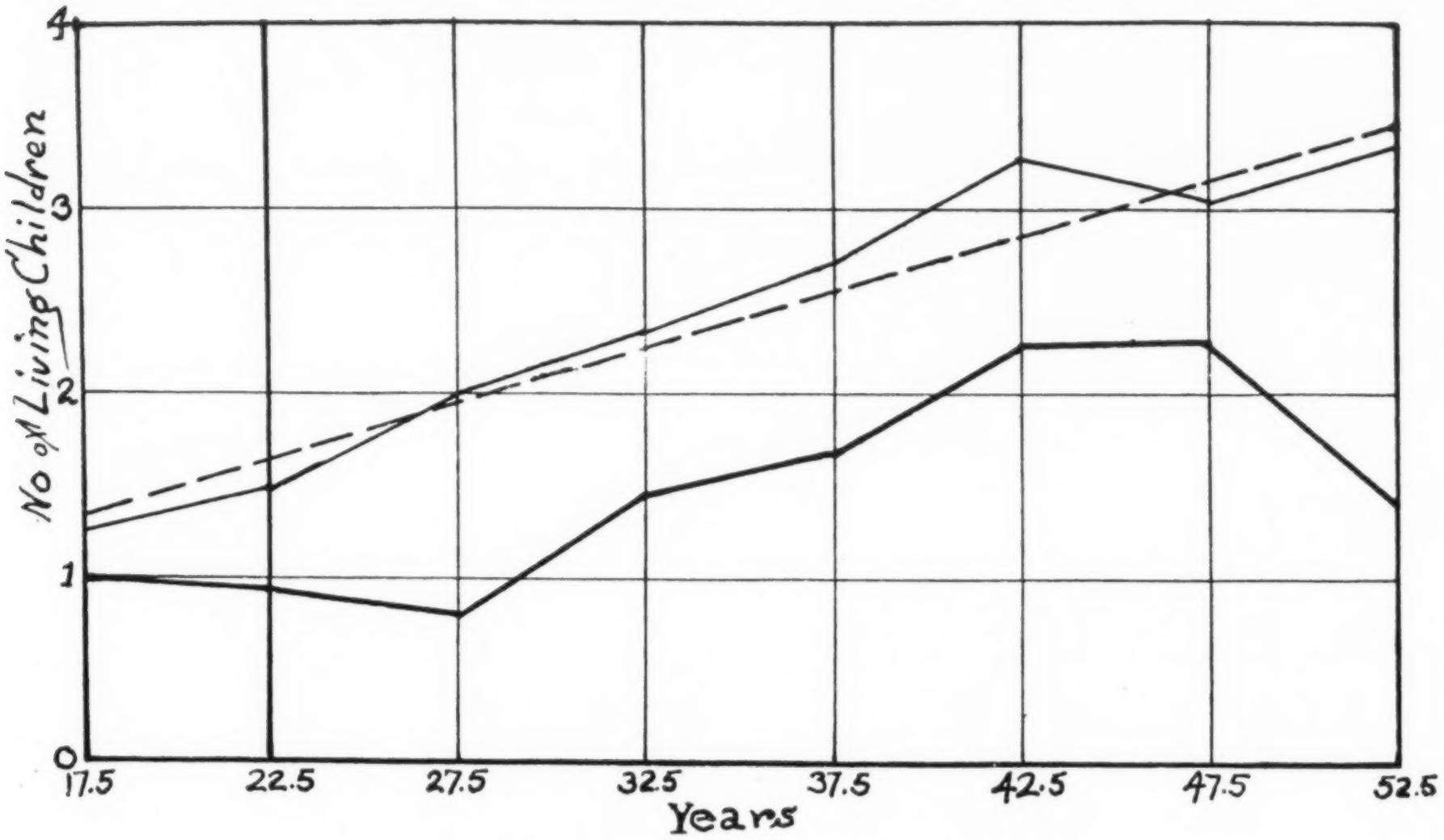
To find what conditions were associated with differences in fecundity among the sterilized patients, the number of living children of those described as Roman Catholics and Protestants, respectively, was compared, as shown in Table VI.

The difference between the males of the two confessions is not significant statistically (it might be if the numbers were larger); that between the females is significant and can not be explained by a difference in age of mothers, although Roman Catholics in this instance include many representatives of the Latin peoples, who are supposed to marry relatively early. The larger families of the Protestants might be due to greater natural fertility, or less contraception; or to a higher rate of infant mortality among the Roman Catholics.

On the latter point, our data show the Protestant married woman had an average of $0.66 \pm .04$ unproductive pregnancies each (that is, child deaths, still births, miscarriages, and abortions), and the Roman Catholics $0.71 \pm .04$ each. The difference, while suggestive, is not significant in view of the probable error, and we felt so little confidence in the completeness of the data that we did not pursue this line of investigation farther.

Family History and Fecundity

We next studied the difference between size of net family (as measured by the number of living children) and the nature of the family history,—whether positive or negative. For the



SIZE OF FAMILIES OF THE INSANE

Figure 10

No. of living children of sterilized insane of each age group (age at operation). Heavy solid line, males, light solid line, females. The light dotted line is the theoretical straight line for the females (equation $y=1.01+.31x$).

present purposes, a positive family history is one in which there is a record of at least one other case of serious mental disease or mental defect in the near ancestry. A negative family history is one in which there is stated to have been no such other case. Histories which gave no particulars on this point were omitted from the tabulation as "unknown."

Married males with positive ancestry had $1.82 \pm .13$ children, those whose ancestry was negative had $1.63 \pm .08$. The difference is not statistically significant, perhaps because of the few cases studied.

Among the women a positive family history went with a mean family of $2.09 \pm .05$ living children, a negative one with $2.45 \pm .04$. This difference of one-third of a child is large enough to be significant statistically, and may be explained by the fact that women who have insane relatives tend to limit their own reproduction, in order to avoid passing on this taint. With the data available, it is impossible to test this supposition, however. The difference might also be due to higher infant mortality among those with insane relatives; greater amount of sterility among them due for instance to venereal diseases; and the like.

Childlessness

Of the married women sterilized, 15 per cent were childless. This proportion is probably no higher than that of the childless women in the population at large, of the same stratum. It might be supposed, since they are relatively young (see Table VII), that these childless women were those who had married only recently, and who were selected for sterilization primarily because they felt they should not bear children. If they were naturally as fertile as the others, their sterilization would be an undoubted gain, eugenically.

But it seems possible that they differ organically from the married women with children. This conclusion is suggested by a study of their menstrual histories. In many cases it

is stated definitely whether menstruation was "regular" or "irregular." While this is a subjective diagnosis, and offers unlimited room for differences of opinion, there seems to be no reason why a different standard of judging should have been used in each of the groups named in Table VIII. The findings among all sterilized women, and our controls, are shown in this table, in which the figures tell the percentage of each group who were stated to be "regular."

The general agreement of the results in these two groups inclines us to think that there is some reality back of the classifications. In both instances, the women who have had children show a higher percentage of menstrual regularity than do the single women, and their superiority over the childless married women is still more marked. It looks as if one of the causes of the childlessness of these married women might be a perturbation of the reproductive system, which also manifests itself in irregular menstruation.

An alternative explanation is that the fact of child-bearing has tended to bring about greater regularity in menstruation. This is undoubtedly true in part; pregnancy, especially in early married life, frequently results in stabilizing the menstrual function.

But the study[4] of W. C. Alvarez and A. Zimmermann, on 1227 women who were not psychopathic, showed a marked relation between sexual normality and marriage, in the sense of our own results. Among the single women, particularly at the early ages, they found a much larger proportion of sexual abnormalities of every kind, including defective development of the pelvic organs, irregular menstruation, masculine distribution of pubic hair, and the like.

Unfortunately, they did not consider separately the married women with children and those without children; but their study does, we believe, offer corroborative collateral evidence favorable to our suggestion that the married women in the sterilized group who have living children are organically different both from the single women and the childless married ones, in the same group. It, too, is of great eugenic significance, in showing the influence of natural selection, exerted through delay in marriage of the sexually abnormal, which must result in a marked diminution of their fertility. In both cases the result tends to preserve the level of sexual normality in the race.

The amount of actual sterility among women who have been at some time legally adjudged insane is equally important from the eugenic point of view. While only 15 per cent of our sterilized married women were childless, 33 per cent of the married controls were childless and, as pointed out above, they are old enough (averaging more than 40 years) to make it certain that most of them will die childless. Presumably this sterility is in part physiological and in part voluntary. Our data do not not permit a differentiation on this point. But in any discussion of the fecundity of the insane woman, it is important to bear in mind the situation exhibited among our controls: one-fourth of them single and of the married, one -third childless. In other words, of an unselected group of female insane, it appears that not much more than half would ever bear children, even without sterilization.

This fact simplifies the problem of eugenic sterilization, for as soon as it is possible to work out methods by which at least a part of the naturally infertile half can be identified, it will be unnecessary to sterilize these, and the saving of time and expense can be applied to extension of sterilization among those who do require it.

Living and Dead Children

Increase in the number of living children, among the married sterilized insane, is associated to a marked extent with increase in the number of dead children (reckoning under this

head all unproductive pregnancies). Undoubtedly the data are not complete; and those for men are still less complete, so that we thought it only misleading to publish the latter. The relationship between number of living children and all other pregnancies, as measured by the product-moment correlation, is:

Sterilizations	.57±.01
Controls	.41±.04

Our data for the mothers of feebleminded patients at the Sonoma State Home were analyzed in the same way, and the correlation between productive and unproductive pregnancies found to be .20 ± .03. Unfortunately we have not discovered any data from superior or even normal families, for comparison.

Many studies[5] have shown that there is a correlation, ranging from about .30 to about .70, between the birth-rate and the infant-mortality rate in an entire community, but the question of the relation between the number of living and the number of dead children in a single family is usually ignored. The correlation is of course made up of two major components which are not easily disentangled: variations in fecundity and variations in infant mortality. These two components are further correlated with each other, producing a situation which, in view of its eugenic importance, deserves, but has not yet received, detailed analysis.

It seems reasonable to suppose that in superior stock the correlation would be negative rather than positive—that is, the more children a family lost, the fewer living ones would remain. If this supposition is well founded, then the high positive correlation among the families of the insane—a correlation much higher even than in the feebleminded stocks of the community—has a marked eugenic significance, pointing not only to the wastage of human material here (natural selection?) but, from the present point of view, particularly to the need for checking this unproductive fecundity for the benefit of all concerned.

Reduction of Fertility

Among insane married women (and largely, men) fecundity is reduced in a number of different ways.

1. The net fecundity is reduced (even though the gross fecundity be much greater, as mentioned in the preceding paragraphs), through spontaneous or induced abortion, stillbirths, and infant deaths.

2. The gross fecundity is reduced by
   (a) voluntary prevention of conception.
   (b) effect of separation from husband when the woman is committed to an institution (similarly through the desertion, separation, or divorce that is found rather frequently in institutional family histories).
   (c) physiological sterility, inherent or acquired. In some psychoses, notably dementia praecox, glandular involvement may be great enough to produce barrenness. In paresis, the effects of syphilis produce a similar marked fall in the net fecundity. One investigation on this point[6] found 33% of the marriages sterile and 20% of the families had abortions, miscarriages, or stillbirths, while dead children also occurred in 20%. The average number of living children per marriage in this series was 1.3, which agrees well with the figures we found in our controls.

Scottish Statistics of D. Heron

The figures which we have found for the fecundity of the insane are much below those reported by David Heron, in dealing with the statistics of a Scottish hospital. He listed 87 families (48 male and 39 female) in which the mean number of living children was 5.18±.21. It is not clear whether his data were unconsciously selected in such a way as to include

an undue proportion of persons with large families. In any event, they were from Scotland, and a generation ago, and we have found no indication in California in the present generation that such families are produced by the insane.

Davenport and Weeks

C. B. Davenport and David F. Weeks published information[7] concerning 158 families of parents, one or both of whom showed a marked "neuropathic condition." They interpreted this rather broadly, including some families that might have been classed as normal by other students. The average number of living children was 4.73, a lower figure than that of Dr. Heron, but still twice that of the California families. It is possible that the method of selection may have tended in some way to include an undue proportion of large families.

It is highly important to know whether the families that produce at least one insane person are larger or smaller than the average; for they are in many cases transmitting recessive traits, in their apparently normal children, which are likely later to reappear and give rise to further mental disease. Our own records throw no light on this point. Dr. Heron gathered data[8] of 1,000 completed families which showed an average of six children each, and led to the conclusion that the insane come from among the most fertile stocks of the entire population.

Need of Statistical Correction

It appears to us, however, that he did not take sufficient account of the fact of selection in his families. They were listed on the basis of having contributed at least one child each to a hospital for mental diseases. Now a large family is more likely to contribute in this way, than is a small family, simply because it has more children. Therefore the large families will tend to be over-represented; and the problem is to deduce, from the observed size of families in the tabulation, what the most probable size of families is in the whole population which they represent.

There is no way of attaining this result with micrometric accuracy[9], but the method ordinarily adopted probably gives at least a much nearer approach to the actual condition found in the whole population. This is illustrated by the treatment of Dr. Heron's data in Table IX.

The probability that a family will have a child in the hospital, is assumed to be in direct proportion to the number of children in the family; hence a new, hypothetical list (Col. 3) of the number of families of each size is made up by dividing the number of families in each instance (Col. 2) by the size of the family (Col. 1). The sum of Column 2 (1,000) divided by the sum of Column 3 (239.05) gives 4.18 children per family as the most probable figure for all families of this sort that have any children at all—a decrease of nearly a third from Dr. Heron's figure of 5.97.

But even this figure is too large, since there will be many families in this group that have no children at all, and therefore do not get into the tabulation. If the proportion of childless families be taken arbitrarily as 20 per cent, then the 1,000 fruitful marriages represent a total of 1,250 marriages, of which 250 (20%) are childless. The proper figure would then be $1{,}000 \times 4.18 \div 1{,}250 = 3.34$ children.

If, then, 3.34 children be taken as the average size of family among these Scottish families that are producing insanity, it is clear that the stock, instead of being one of the most fertile in the population, is scarcely maintaining itself.

Many other published data must be corrected in the same way, to avoid drawing a distorted picture of the real facts. Thus C. B. Davenport and David F. Weeks present informa-

tion[7] concerning 24 families, averaging 5.37 children each, of parents both normal who produced at least one epileptic child. Corrected as above, these families show an average of only 3.22 living children; and if allowance is made for 20 per cent sterility, it appears likely that epileptic stocks in that part of the United States actually produce about 2.57 children each. If these families were complete, it is clear that the stock does not reproduce itself.

Further information along these lines is highly necessary to the future application of eugenic sterilization, and it is hoped that many hospitals will publish their own statistics. It may be found that the California figures are low, in comparison with those of other states. The question should be settled by abundant and authentic information from all parts of the Union.

Conclusion

It is regrettable that on a matter of such far-reaching national importance, data are not now abundant enough to furnish a conclusive answer. Our information, which we feel represents with reasonable accuracy the conditions in a single state, California, shows clearly* that the insane† do not perpetuate their own numbers. The widespread though vague clinical idea that insanity tends to "breed itself out" in three generations accords with our findings, in so far as it indicates that there are few insane persons whose parents and grandparents were both also insane. It is, however, mischievously false, if it is interpreted to mean that insanity would rapidly disappear from a population of its own accord, if nothing were done to check it. It does, perhaps, tend to disappear in a sense—only to reappear again later in many cases, among the descendants of the affected person.

If insanity is increasing in the United States (which has been both affirmed[10] and denied[11], it can scarcely be increasing through the reproduction of persons who have at some time been legally adjudged insane. On the other hand, such persons do contribute to each generation a large number of children who are definitely marked for psychoses; therefore sterilization is a definite gain eugenically as well as, in many instances, of great benefit to the patient therapeutically by preventing the strain of pregnancy.

We are concerned here only to point out that the sterilization of those who are committed to hospitals for mental disease is not, of itself, an adequate measure to deal with the problem of the production of insanity. We shall deal with the inheritance of a tendency to mental disease in a future paper; here we shall limit ourselves to pointing out the immediate conclusion of the greatest eugenic import, namely, that a large part of the insanity comes from parents who, while not themselves insane, have insane relatives and are therefore carrying recessive genes.

In so far, then, as the development of a psychosis is dependent on inheritance, any mental hygiene program which attempts to prevent the development of psychoses must deal

*To save space, we have sometimes omitted publication of the full data on which our averages or correlations are based. In any such case, we shall be glad to send a copy of the original data to any qualified student who wishes to use it. All our material is accessible for examination in the office of E. S. Gosney, 26 N. Marengo avenue, Pasadena, California.

†Throughout this investigation, we are using the term "insane" in the only sense in which it has any real meaning, namely, to describe a person whom a court has declared, after due process of law, to be of unsound mind, and committed to an institution. There are many persons in the general population who have mental diseases, but if they have not been adjudged incompetent by a court, they are not "insane" and this study does not apply to them. Insanity is purely a legal concept, not a biological term. In short, when we speak of the insane, we speak of those who are or have been patients of hospitals for the insane.

with these families in which the disease is latent, even more energetically than with those who find their way to the state hospitals.

If sterilization is to be a part of such a program, there must be some means by which persons who are not themselves insane or likely to become insane, but who will probably have insane children or grandchildren, can undergo sterilization if they desire.

Summary

1. Among persons sterilized in the California state hospitals for mental diseases, the mean size of family is: married men 1.64 living children, married women 2.03; all men 0.61, all women 1.85.

2. A married unsterilized insane woman will, on the average, produce a living child every 15 years.

3. The fecundity of the unsterilized insane studied as controls is lower than the above figures. It appears that only one-sixth of the married female controls will produce enough children to increase the size of their group.

4. In general, no large group of institutional insane in California will produce enough children wholly to reproduce itself, even if it were not sterilized, the completed family being only two or three children. (Higher figures have been reported elsewhere.)

5. Most of the insane are born of parents who are not themselves insane. The fecundity of these parents appears from other studies to be a little greater than that of insane parents. Any extensive program of eugenic sterilization should find a way to reach these parents, at least through making voluntary sterilization possible, if it is to achieve maximum results.

6. There are no significant differences between the sizes of families of sterilized women with the different types of psychoses.

7. Among sterilized married women, the Protestants were found to have about one-third of a living child more than the Roman Catholics. There was no demonstrated difference among the men.

8. Women who had no near relatives insane had each about one-third of a living child more than those who had a family history of mental disease. There was no demonstrated difference among the men.

9. Among insane women in general (married controls of this study), about one-third are childless. Reasons are given for believing that this represents in part a physiological sterility, with which menstrual disturbances are associated.

10. Insane women who had the largest number of living children also tended to have the largest number of dead children.

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TABLE I

No. of living children of married patients who were sterilized, by age of parent at time of operation.

Ages	Males			Females		
	No. of fathers	No. of children	Mean family	No. of mothers	No. of children	Mean family
15-19	4	4	1.00	26	33	1.27
20-24	14	13	0.93	127	200	1.49
25-29	39	32	0.82	200	402	2.01
30-34	56	80	1.43	249	585	2.35
35-39	82	137	1.67	169	456	2.69
40-44	59	134	2.27	54	177	3.28
45-49	31	70	2.26	19	58	3.05
50 up	16	23	1.44	8	27	3.37
	310	493	1.64	852	1938	2.03

TABLE II

Number of living children born to married controls, by parent's age at admission

Ages	MALES			FEMALES		
	No. of fathers	No. of children	Mean family	No. of mothers	No. of children	Mean family
15-19				1	1	1.00
20-24	1	2	2.00	4	5	1.25
25-29	4	5	1.25	12	11	0.92
30-34	11	4	0.36	17	9	0.53
35-39	14	17	1.21	15	20	1.25
40-44	18	20	1.11	28	54	1.93
45-49	10	18	1.80	23	36	1.56
50 up	28	37	1.32	43	78	1.81
	86	103	1.19	143	214	1.50

TABLE III

Fecundity of married female controls over age 40 at admission.

Size of family	TOTAL PREGNANCIES			LIVING CHILDREN		
	No. of mothers	Percent of mothers	Total no. of children	No. of mothers	%	Total no. of children
0	18	19.15	0	26	27.67	0
1	19	20.21	19	23	24.47	23
2	13	13.83	26	17	18.08	34
3	10	10.64	30	12	12.77	36
4	9	9.58	36	9	9.58	36
5	12	12.77	60	7	7.43	35
6	3	3.19	18			
7	3	3.19	21			
8 up	7	7.44	56			
	94	100.00	266	94	100.00	164

TABLE IV

Diagnosis	Male		Female	
	Number of pregnancies	Age at operation	No. of pregn.	Age at operation
Dementia praecox	1.59±.11	37.45±.07	2.68±.08	32.10±.29
Manic depressive	1.75±.12	37.35±.48	3.35±.06	31.40±.21
Feebleminded, etc	2.18±.34	37.25±1.07	2.56±.15	28.71±.47
Alcohol, drugs, GP	1.70±.25	38.56±.60	3.21±.37	30.72±1.14
Epileptic	2.00±.31	37.50±1.01	2.45±.25	30.43±.87
Miscellaneous	2.63±.38	40.50±1.35	2.79±.17	32.49±.67

Table VI

Number of living children to married sterilized patients.

	Number	Mean age at operation	Mean size of family
MALES			
Catholic	44	37.95±.78	2.29±.21
Protestant	75	37.49±.48	1.98±.16
FEMALES			
Catholic	272	31.60±.25	1.49±.07
Protestant	251	31.25±.28	1.84±.07

TABLE V

Mean number of living children for each married woman.

Diagnosis	Sterilizations		Controls	
	No. of mothers	No. of children	No. of mothers	No. of children
Dementia praecox	250	2.16±.06	48	1.63±.15
Manic depressive	444	2.29±.05	40	1.50±.17
Feeble-minded, CPI, etc.	76	2.16±.12	4	2.25±.96
Alcoholic, GP, drugs	14	2.28±.30	18	0.95±.21
Epileptic	32	2.00±.17	3	1.25±1.47
Miscellan.	81	2.23±.15	33	1.82±.18

TABLE VII

Mean age of sterilized patients according to reproductive history

GROUP	MALES	FEMALES
Single Sterilizations	32.07±.04	26.87±.05
Single controls	33.85±.66	34.90±1.02
Married sterilizations no children	37.44±.09	28.30±.08
Married controls no children	42.90±.91	40.45±.88
Married sterilizations with children	37.44±.07	32.29±.03
Married controls, with children	43.60±.75	43.65±.69

TABLE VIII

Menstruation Histories
Percent of each group reported to be "regular"

	Sterilizations	Controls
Single women	63.06±2.60%	71.43±6.20%
Married, with children	76.68±1.50	91.23±2.60
Married, no children	63.79±4.20	59.09±6.60

TABLE IX

D. Heron's data, completed families of parents of insane patients, with statistical correction.

1	2	3
No. of offspring	No. of families	(2÷1)
1	48	48
2	54	27
3	124	41.33
4	103	25.75
5	154	30.80
6	106	17.66
7	118	16.86
8	118	14.75
9	64	7.11
10	33	3.30
11	33	3.00
12	18	1.50
13	18	1.38
14	3	0.21
15 up	6	0.40
	1000	139.05

Better Sheep Result of Grading Practice

GRADING and selling cooperatively has helped materially to improve the livestock of Alleghany County, Va., according to information received by the United States Department of Agriculture from County Agent S. B. Surber. Referring to the grading and sale of sheep, Mr. Surber contrasts the old method with the new. "In the old way," he says, "it was the same price to everybody, but under the grading plan the best lambs bring the most money. Our farmers soon learned that if a neighbor had better lambs he received a larger check. This induced them to take notice of what neighbors were doing. It was not long before those with inferior breeding stock obtained better bucks and also gave their sheep the stomach-worm treatment. Briefly, grading has done a great deal toward increasing the use of better bucks and toward the better care of lambs."

The local association has been shipping about 4,000 lambs each year with satisfactory returns to owners.

U. S. Dept. Agric.

Mutations of Zinnia and Aster

THAT mutations are not necessarily in the direction of betterment is well known and that they may even be self-limited is well shown by the

freak zinnia illustrated herewith. At first it was thought that the development of supplemental heads in the axils of the disc leaflets or chaff might have been due to the sting of some insect since in every other respect the plant appeared entirely normal, but all subsequent flower heads developed in the same way, indicating that the character was inherent. Morphologically considered the buds theoretically present in the axils of bracts and chaff, but normally never developing, have here developed axes of their own, forming headlets of non-ligulate flowers. As only ligulate flowers bear fully developed seed, it is obvious that this mutation, if such we may consider it, is self limited and must disappear as quickly as it came.

The china aster here illustrated appeared in a planting at Chevy Chase, Maryland, during the season of 1927. The quite unusual aspect is due to the fact that all the ray florets have greatly reduced ligules so that they present

a mere border of lavender about the yellow disc. It is here contrasted with a normal flower as they occurred on the remaining plants. While such an abnormality can hardly contribute anything of value to the ornamental garden there is a possibility that it might be made the basis of an interesting study in genetics. With this in mind seed of this plant has been kept and will be gladly turned over to some student who may care to grow it to determine its genetic behavior.

John Marion Schull.

A Reply to the Criticism of "Animal Breeding" by H. C. M.

THE March 1927 issue of THE JOURNAL OF HEREDITY contains the somewhat tardy criticism of "Animal Breeding." H. C. M. is apparently laboring under the impression that the book was intended to be, or should have been, a book on advanced genetics, much as solid geometry follows plain geometry. Apparently he is unaware of the fact that in a large number of our agricultural colleges a course in animal breeding follows a course in genetics much as a course in feeding and management follows a course in nutrition. I have read many books on the feeding and management of livestock and criticisms on the same; and I have yet to find where the student of nutrition has criticised an author of such a book, because the book was not a book on advanced nutrition, or because in relating the results of feeding experiments all the nutritional factors involved were not explained. The writer belongs to that optimistic group who looks forward to the day when animal breeding may be taught as an advanced course in genetics. That day has not as yet arrived, and neither will it arrive until many more studies, such as, Gowan's study of milk secretion and Warwick's study of hernia have been completed. Even though the above-mentioned facts are new to H. C. M., he should have realized that "Animal Breeding" was not intended to be classified as a genetic volume when he found that only 82 pages out of 309 were classified as "Heredity." It may ease his mind, however, to know that the author plucked some 50 pages from the part devoted to heredity when it was decided that the book should appear as a companion volume to Jones' "Genetics in Plant and Animal Improvement," the reasons being to avoid overlapping Jones' work as much as possible, and to try and prevent anyone making the mistake as to the purpose of the book, which H. C. M. made.

H. C. M. quarrels with the use of livestock illustrations in a book on livestock and to livestock men. Would he use flowers to illustrate Mendelian principles to the breeder of livestock, and rabbits to the horticulturist? Such a method might add novelty, but I question either its appeal or wisdom. At this point H. C. M. suggests the use of Mendel's experiments with peas, and one wonders if he is not aware that several authors of very good books on genetics have seen fit to explain Mendelian principles by other means.

The critic also quarrels with the writer on the value of Galton's Law of Ancestral Heredity to practical animal breeding. The breeders of larger animals are chiefly concerned with quantitative characters, and, apparently, most of these characters are the result of multiple factors which we as yet know very little about so far as larger animals are concerned. One of the important problems of the teacher of animal husbandry is to give his students the tools with which they can go to the farmer and drive home both the need and the means of improving the rank and file of our farm livestock; for it is of far greater economic importance that our general average be improved than that our "tops" be further improved, and this is not minimizing the value of

the latter. Galton's Law has been used very effectively to illustrate, to the man raising farm animals, how herd, stud, and flock improvement can be, is being, and has been made. It has also served a useful purpose in giving weighted values in judging pedigrees. Rather interesting at least on this point are some of the correlation coefficients reported by Gowan in "Milk Secretion" for the influence of ancestors on milk yields and butter-fat percentage of dairy cows. On page 323 we find the following:

"Coefficients showing the correlation between the male ancestors and the productivity of the granddaughters.

Type of ancestor	*Milk yield*	*Butter-fat percentage*
Sire	0.521	0.526
Paternal Grandsire	0.245	0.339
Maternal Grandsire	0.350	0.390

The following is taken from page 332 of the same volume:

"Correlation coeffcients for the influence of the parents and grandparents on the milk yields and butter-fat percentages of the granddaughters.

Type of ancestor	*Milk yield*	*Butter-fat percentage*
Dam	0.497	0.413
Paternal grandam	0.258	0.091
Maternal granddam	0.307	0.192

On page 325 Gowan states: "These data point to the conclusion that the grandsire is only one-half as effective in controlling the milk yield or butter-fat percentage of the granddaughter as the sire or, for that matter, the dam." And on page 332 in referring to the last table given above Gowan states: "Table 158 brings out the fact that the correlation coefficients of the grandparents, productivity with that of the granddaughters is half or slightly less than half that of the parents with their daughters."

This would indicate at least that the weighted values given the various ancestors by Galton are likely not so bad in their broad application to animal breeding.*

Our critic dismisses as practically useless the part devoted to a review of the Sni-a-Bar beef breeding experiment, the Ames and South Dakota dairy breeding experiments, Dr. Schroeder's studies of infectious abortion, the Wisconsin and Ames studies on nutrition of the dam during gestation as a factor influencing the health of the offspring, the New Mexico studies of reproduction of heifers under range conditions, and others. Is H. C. M. unaware that thousands of farmers and breeders gather annually at the Sni-a-Bar farm for both inspiration and guidance? Is he unaware that the Ames experiment has served as guidance to many thousands of dairy breeders? Is he unaware that the annual loss from infectious abortion in North America is close to one hundred million dollars? Is he unaware of the losses farmers and ranchers suffer because of the nutritional and environmental factors with which their breeding animals contend?

The writer is taken to task for two typographical errors. No writer likes typographical errors, even though, *parva leves capiunt animas;* and for one I apologize. For using albinoism in place of albinism, I suppose, I should apologize, since H. C. M. says it is an error; but I find the same mistake made on page 65 in Funk and Wagnalls' "New Standard Dictionary of the English Language," published in 1913 and I find it made again on page 51 in Webster's "New International Dictionary," published in 1926. This is not the first time, I have noticed that H. C. M. is especially keen on typographical errors, and so I did sincerely hope that he might be classified among those, all too few, who publish with a freedom from these trouble makers. But, alas, *parturiunt montes,*

*Since the above was written, the August, 1927 issue of "The Journal of Heredity" contained a very clear and straightforward presentation of the value of "Fractions of Blood" as an index of the genetic constitution of an animal in the article, "Percentage of Blood and Mendelism," by Jay L. Lush.

one appears in the first line of his criticism in using the writer's first name and another in the eighth line in using the writer's last name. Maybe there are more, I do not know. I quit looking for them at this point, for two in a one-page review would give 618 in a 309-page volume. (How careless these stenographers and typesetters have become in this day of speed.)

Mistakes in a publication are serious; but in a review, due to either carelessness or lack of comprehension, they are more than serious. This is the real reason for this reply.

LAURENCE M. WINTERS,
University of Saskatchewan.

If any defense of my review of "Animal Breeding" is necessary, may I be permitted to quote from the review of the same book, which appeared in *Nature* (London):

"With these notions as a foundation, it can be understood that when the author comes to discuss such topics as inbreeding, crossbreeding, and selection, he is neither clear nor stimulating. For the rest, the book is of the "scissors and paste" type, lacking that unity of presentation which comes of sound and critical thinking." *H. C. M.*

Plant Hunting

WILSON, E. H., Plant Hunting, Vol. I, pp. xxix + 248, Vol. II, pp. 276, illustrated. Price, $15.00. Stratford Co., Boston, 1927.

IT is a pleasure to call attention to another book of unusual value and interest to plantsmen. Within the last two years have appeared two manuals of cultivated plants, the only ones ever published. The author of the present volumes is the plant explorer of the Arnold Arboretum and has probably been longest in actual field work of anyone in that profession now living. He must be credited with having added largely to the list of plants under cultivation in America. The books are popular in style but will be invaluable to all interested in the improvement of ornamental and garden plants (nurserymen especially) and those interested in the history of plants.

Volume I comprises two parts, Africa (South and Central), Australia and New Zealand; and Volume II, the Tropics and the Orient. The history of plant exploration in each part of the world is touched upon and a description is given of the flora of possible value in America. After reading several of the chapters of the second volume, in which some of the personal experiences of the author are recorded, one wishes that he might have recited more of the stirring incidents of plant hunting. Plant breeders and plantsmen of many kinds will live with the author in his description of the introduction of the Regal Lily and of the Poppywort. All students of botany might read with benefit the chapters telling of the search for these plants.

Though the books are avowedly not technical contributions it would seem that they might have been made more helpful if some references had been included.

The reviewer would have been glad if the author had given more space to China and Chinese plant hunting, which he knows so well. Hundreds of agricultural explorers have penetrated the more remote regions of the world and it is Wilson's belief that the day of the professional plant hunter is drawing to a close. Notes that he constantly gives of the scores of plants, in every land, of potential value for the different conditions to be found in this country, and especially for California, increase interest in the profession and give the impression that there is still much to be done. Systematic plant breeding, in which the breeder requires the finest selected living plants of the species of a group, emphasize the increased need for the professional plant hunter of a slightly more specialized kind. *G. M. D.*

TORTOISESHELL TOMCATS AND FREEMARTINS

A Note on the Occurrence of Fused Placentae in Cats

T. Hume Bissonnette
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IN 1920 Doncaster[8] suggested that the sterility of the rare Tortoiseshell Tomcats may be due to sex-reversal by the action of a male sex-hormone during foetal life in the same way as in the freemartin in cattle, as shown so conclusively by Lillie[12,13], Bascom[3], Bissonnette [4,5,6], Keller and Tandler [11], and others. He suggested that the reversal might be carried a step farther and produce morphological but non-functional males, the sterile Tortoiseshell Toms. In that year he and Bamber began a search for fused placentae in cats, with the vascular anastomosis between the placental circulations necessary to make this possible. After his death this search was continued by Bamber[1] who reported in 1922 on the negative results of the investigation. Among some 253 cat placentae examined she found some placentae coherent in the uterus, yet she was of the opinion that no confluence of blood vessels from the two embryos occurred in her specimens. The thin membranous parts of the chorions were coherent but the highly vascular zonary placentae proper, with villi, were not fused completely enough to give the vascular connection necessary to produce this type of sex-reversal. She states, however, that she sees no reason to doubt the possibility of such union as she found being carried a step farther and producing the confluence required or postulated by Doncaster. She thinks, however, that the sterility is conditioned by genetic or chromosomal abnormality and in a later paper she and Herdman[2] offer evidence in support of the genetic basis for the occurrence of these sterile males but criticize the explanation offered by Tjerbbes and Wriedt[16] along somewhat similar lines.

Little[14] discussed the suggestion of Doncaster and discarded it in favor of a genetic explanation for the sterile males. He suggested non-disjunction of the sex-chromosome rather than hormonic sex-reversal.

Hughes[10] has shown that in swine the fusion of placentae is accompanied in rare cases by the same type of intersexuality as is found in the freemartin of cattle. Hoadley with a single case, demonstrated at the meetings of the American Association for the Advancement of Science in Philadelphia December, 1926, supports Hughes' findings. These cases show beyond question that it is not alone among cattle that such a mechanism for the transfer of sex-hormone from embryo to embryo in utero is to be found, as was supposed by many.

Hartman[9] mentioned a case among dogs, reported to him in a private communication by Professor H. M. Evans of California, in which two very young embryos were found in an apparently single placenta, which he considered to have resulted from fusion of two originally discrete placentae crowded together in the uterus. In this paper Hartman also refers to some other cases reported long ago, before the true nature of the freemartin was recognized, in which some abnormalities in man and the dog were classed as of the freemartin nature.

Bonnevie[7] reported on a sterile female tortoiseshell cat which resembled the freemartin in some respects, but she accounts for the condition on the basis of chromosomal disturbances. She cites Oshima and Oyama[15] as also account-

ing for the same type of modification on the same basis quite independently.

The want of a case of undoubted complete fusion of placentae in cats seems to have caused investigators of the sterile and unexpected Tortoiseshell Tomcats to seek to explain the abnormality on a genetic basis and postulate chromosomal aberration. There is as yet little agreement as to just how this is brought about or what form it takes. There would seem to be a lack of cytological evidence in its support.

In support of the suggestion of Doncaster the following case is described in which it is believed that the postulated mechanism for the sex-hormone transfusion between the embryos in utero in cats is realized.

In opening up a cat last February to show the vascular changes induced by pregnancy and the changes in the female reproductive organs concerned, one was chosen that had been killed and injected with the usual carmine-starch-formalin mass during the preceding summer, and kept in the vats in formalin solution. A demonstration of the type of placenta and of the relations of the embryos to it was also to be made.

On opening the abdomen and exposing the uterus, six swellings, three on each side, were found. In the left horn the swelling next the ovary was very close to it and somewhat larger than any of the others in the uterus. It was therefore chosen for opening. The uterus was cut lengthwise and the placenta peeled out without injury and laid on a watchglass, while the lining of the uterus was inspected. It was then carefully slit lengthwise along the meridian between the open ends of the placenta where the chorions remain thin and the sides were carefully laid back to expose the contents. Instead of the expected single cavity with a single embryo inside, two cavities with two embryos about 14 and 14.5 mm. crown-rump length were found. The two compartments or chorions were separated by a membranous partition similar to the membranes at the ends of the barrel-shaped placenta itself. Each embryo was in its own amnion and had its own proper yolk-sac, and the umbilical chords were attached to the placenta at some distance from each other. The allantoic blood vessels could be seen plainly, leading out from the cord over the inside of the placenta.

The first thought was that this was a case of identical twins in cats. But the partition between the cavities in which the embryos were found made it evident that there were two chorions as well as two amnions. Careful inspection of the outside of the placenta showed a faint groove extending round it like an equator midway between the thinwalled ends and at the same level as the thinwalled partition between the chorions. These facts and the presence of a slight ridge on the inside opposite the groove on the outside, as seen on careful inspection of the cut edges in this region, and affording attachment for the membranous partition, made this appear to be a case of practically complete fusion of placentae in cats This was confirmed by a microscopic study of a small piece about 6 x 4 mm. including part of the region of union, which was cut out, sectioned in paraffin, stained in Heidenhain's Iron Hematoxylin, and mounted in balsam. The union was found to be so complete that no separation of the circulations from one to the other embryo in the villi either of the embryonic or of the maternal part of the placenta, could be made out. The villi were continuous on both maternal and embryo sides across the place of union, and both maternal blood as shown by the injection mass, and foetal blood as shown by the presence of blood-corpuscles without the injection mass, appeared to circulate through all the villi. No signs of any barrier were evident in the part sectioned.

On sectioning the embryos, they were found to be still in the so-called sexually indifferent stage. Sexes could not be distinguished.

Complete data and figures for this case will be published in a later paper,

but it is thought worth while to report in this preliminary paper some of the details to show that the conditions postulated by Doncaster can be and are sometimes realized in cats and that the union may be very complete even before the time of recognizable sexual differentiation, just as was shown by Bissonnette[4] for cattle even as early as the 10 mm. crown-rump length stage. Search for other like cases is in progress, but to date it has yielded no more cases. However this one case alone shows that if the embryonic testis in cats, as in cattle, develops a male hormone capable of causing sex-reversal in female embryos receiving such hormone, the necessary mechanism for its transfer to such female is sometimes realized, and therefore partial sex-reversal is possible. Miss Bonnevie's case might fit in with such a process. In cattle, freemartins sometimes have descended testes even though the external sexual differentiation appears very early and freemartins are seldom if ever externally male in genitalia. These conditions may not obtain in cats and external modification under like stimuli may be possible. Further data are needed to decide this point and settle the question as to whether the Tortoiseshell Tomcat may or may not be an intersexual female with descended testes and even modified external genitalia. This however does not exclude the possibility of genetic intersexes among Tortoiseshell cats. It does however widen the field in which hormonic intersexes may be looked for and expected, from the ungulates over into the carnivores.

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Race Differences in Blood Agglutinins

WALTER SCHEIDT, RASSENUNTERSCHIEDE DES BLUTES MIT BESONDERER BERÜCKSICHTIGUNG DER UNTERSUCHUNGSBEFUNDE AN EUROPAISCHEN BEVÖLKERUNGEN. Pp. 109. Price, 4.80 marks. Leipzig, 1927.

IN recent years much attention was paid by geneticists and anthropologists to the problems of isohemagglutination, to the mode of inheritance of the different blood groups and their importance as a race character. It is this latter point that Scheidt treats in his pamphlet. The principal question which he discusses is whether our present knowledge concerning the distribution of the different blood groups among different populations justifies the belief that they can be used satisfactorily for the classification of human races. One requirement, of course, for this is fulfilled: the blood groups are inherited traits. Of this we have conclusive evidence although the opinions of different authors are still at variance concerning the mode of their inheritance. Further investigations are needed definitely to decide whether two pairs of independent factors or three pairs of multiple allelomorphs determine the genetic expression of the blood groups, and further information is needed concerning the sexual differences in the distribution of the blood groups and concerning changes in the blood groups during adolescence.

In a great many tables Scheidt presents a critical analysis of the data contained in the literature concerning the distribution of the blood groups in different populations from the standpoint of their usefulness for ethnological problems. Although the number of such reports is relatively large, scarcely one of them fulfills all the necessary requirements. The principal reason for this is that the data usually do not fairly represent a given population but by the methods used are pictures of selected groups only.

Comparing the results for German populations, the author is forced to state that there are no indications that the determination of the blood groups can be employed for a racial classification of the central European populations. The same seems to hold for the data for northern European, Italian, Polish and Russian populations. It is true, however, that there is a significant difference between the distribution of the blood groups among Eastern European and North- or South-western European populations; the percentage of the B groups increases from West towards East. This was explained by different authors as due to a regionally different origin of the blood groups. Scheidt demonstrates that the present distribution of the blood groups cannot be explained satisfactorily by migration and mixture of races with different blood groups only, but that selection has to be taken into account.

Although there is no reason to believe that the blood groups can be used for a criterion of racial identity, they constitute a valuable character for the description of populations if they are used together with such other traits as have been used for this purpose in the past (color of skin, type of hair, index of skull and so on). Scheidt's discussion of the value of the blood groups for ethnological investigations is very clear and concise and undoubtedly will help to prevent unwarranted generalizations and to stimulate new investigations. It is a very timely and commendable contribution, based upon a thorough analysis of the known facts.

Walter Landauer.

NATIONAL RESEARCH COUNCIL PROJECTS IN HEREDITY

Report to the American Genetic Association of the Work of the Division of Biology and Agriculture which May Be of Interest to the Society

THE National Research Council, a cooperative organization for the promotion of scientific research, is founded on the principles of representation through the national scientific societies. Taking the Division of Biology and Agriculture as an example closest to our interests, fifteen societies in both the pure and the applied fields of biology are given opportunity (1) to select someone from their respective memberships to constitute, with several members-at-large chosen by the Division as a whole, the membership of the Division; (2) to bring to the Council the research needs in their own fields which this body may be in position to encourage; and (3) to take an active part in the general work of the Division. The history of the Division has indicated that this plan for serving the scientific groups of the country has worked out unusually well. Already a dozen or so outstanding achievements have resulted and are now operating independently as major activities in the realm of science. The work of the Council, like its membership, is a continuing process and each year as certain projects branch out to stand alone (more or less) new ones arise. Consequently there is at all times a large list of active projects. Those that may be of interest especially to the members of the American Genetic Association are briefly reported below:

1. *Committee on Animal Breeding.*—This Committee has been operating since 1924, first under the chairmanship of Doctor L. J. Cole, and beginning with July, 1926, with Mr. E. N. Wentworth as Chairman. The organization of the Committee now includes four sub-committees as follows:

Sub-committee on Survey of Research, J. A. Gamble, Chairman.

Sub-committee on Methods of Analysis, W. L. Gaines, Chairman.

Sub-committee on Standardization of Records, L. J. Cole, Chairman.

Sub-committee on Finance, E. N. Wentworth, Chairman.

In the report of a meeting of the Committee held in November, 1926, it is noted that the Sub-committee on Methods of Analysis has decided to specialize on a few important lines each year. The two major problems now before it are (1) correcting production for age in dairy cows, and (2) the development of methods which will provide an accurate test and measure of the worth of the dairy sire. Doctor J. W. Gowen has been engaged in the preparation of a manuscript summarizing the work of the sub-committee the previous year in working out age corrections in dairy cows. The Sub-committee on Survey of Research, reports the publication of a paper entitled "Investigations in Animal Genetics at the Experiment Stations." by Mr. George Haines of the Office of Experiment Stations. This is in direct line with the work of the Sub-committee, since it represents the first step in attempting the tabulation and correlation of animal genetic research. Professor Gamble indicated that the plan of the Sub-committee for further service was to ascertain the several projects in animal breeding which are being conducted at various experiment stations, so that the information may be available for general use.

2. *Committee on Family Records.*—This committee was established at the request of the Huntington Family As-

sociation, who desired an advisory and scientific committee in the National Research Council to help the Association in its proposal to inaugurate a scientific record -keeping of the physical and mental characteristics in the successive generations of the family. After a careful review of the plan and the method of attacking this problem, the Council through this Division gave its endorsement and the Committee has been in existence since 1922. Doctor Clark Wissler, of the American Museum of Natural History and Yale University, is the present chairman, and H. H. Laughlin (Cold Spring Harbor), Ellsworth Huntington (Yale), E. L. Thorndike (Columbia), Raymond Pearl (Johns Hopkins University), and Vernon Kellogg (National Research Council) are serving as the members of the Committee. Some funds for the Committee's work have been secured and a comprehensive questionnaire has been prepared for obtaining the desired records. The program of work as outlined will require a period of about five years for completion.

5. *Biological Abstracts.*—Since the establishment of the Council the Division has been concerned in a study of publication needs, with a view to attempting to meet some of these. A first step in this direction was the launching of International Biological Abstracts. This abstracting journal expects to cover the whole field of biological and closely related sciences.

6. *Committee on Research Publications.*—The Division has further been concerned with a study of the needs among scientific biological journals for the publication of original research articles. Doctor C. E. McClung has been chairman of this committee during the past few years, but since he is out of the country for the full year and it is felt desirable to push this study this winter, the committee is being reorganized.

7. *Fellowships growing out of activity of the Division.*—In addition to a number of special fellowships, funds for which have been made available at various times for the support of particular pieces of research, there has been operating since 1923 a major series of fellowships known as the National Research Fellowships in the Biological Sciences and designed primarily for the training and development in fundamental biological science of individuals beyond the doctorate stage. These fellowships do not include within their scope agricultural research workers, except as they may be working along fundamental lines. The need for fellowships in agriculture is keenly appreciated and it is hoped that eventually support for such fellowships may be secured.

There is within the Division one special fellowship which may be of interest to members of the American Genetic Association, namely, the hard seed fellowship. This is the first of a series of seed germination problems proposed as fellowship studies and for which support was desired. It was maintained for three years, from 1924 to 1927, at the University of Wisconsin, with Dr. David Schmidt as the recipient, the funds being at first supplied by the American Seed Trade Association and subsequently by the Wholesale Grass Seed Dealers' Association (now known as the Farm Seed Association of North America). Beginning with the fall of 1927, the latter organization has continued the fellowship at a lesser stipend, with a graduate student working half-time at the University of Wisconsin under the direction of Doctor B. M. Duggar. A report on the work done by Doctor Schmidt is in preparation at the present time

Another fellowship which is maintained under the Division of Biology and agriculture is that for the encouragement and development of research work among the negro race, known as the Rosenwald Fellowship. Funds for the maintenance of this were supplied by Mr. Julius Rosenwald of Chicago from July 1, 1920, to June 30, 1927, inclusive, and with

the beginning of this present fiscal year the General Education Board has taken over the support of the Fellowship for a period of four years. The holder of the fellowship is Doctor E. E. Just of Howard University. Doctor Just's research is concerned with fertilization studies.

8. *Commission on the Standardization of Biological Stains.*—This organization is now independent of the National Research Council except that the latter acts as its fiscal agent. It has established in 1922 for the development in this country of standardized stains. In addition to the testing the Commission does for commercial concerns looking toward certification of their stains, the Commission has published a book entitled "Biological Stains, a handbook on the nature and uses of the dyes employed in the biological laboratory," and beginning with January, 1928, a quarterly journal was issued, containing original articles on stains, new uses for them, and a new microscopical technique in fields where stains are used. The work of the Commission in these years since its establishment has been financed through the Chemical Foundation primarily, small grants from dealers in stains received from time to time, and through fees resulting from the certification of stains.

9. *Institute for Research in Tropical America.*—The Institute for Research in Tropical America was organized, under the auspices of the Division of Biology and Agriculture, in January, 1921, for the purpose of promoting "research in Tropical America, by exploration, by the establishment of laboratories and research stations, by coordinating the efforts of cooperating institutions, and by such other methods as the Institute may adopt. While the work of the Institute may embrace scientific investigations of any nature relating to Tropical America, it will include primarily biological research in relation to the animals, the plants, the people, the geology and soils, and the climate, all with due reference to such aspects of applied biology as medicine, agriculture, forestry, and fisheries."

Since its organization the Institute has concentrated its energies largely upon the establishment of the Barro Colorado Island Biological Station in the Panama Canal Zone. The assistance of the National Research Council was sought in endorsing this project to the Government of Panama to bring pressure to bear on securing the site desired for this Station. The Council gave its endorsement. The reserve was subsequently set aside, and with Doctor Thomas Barbour in charge, plans for the establishment of the station went forward without delay. It has been a going concern since the summer of 1923 and has received financial support from various sources. Seventy-five persons at least have made use of the facilities at the laboratory during the few years of its operation.

10. *Committee on Tropical Research.*—This is a reorganization of what was formerly the Committee on Sub-tropical Biological Institution, which in turn was an outgrowth of other committees of the Division concerned with tropical biology and agriculture. It has been expedient to change the character and personnel of the committee to accord with changes in the character and scope of the problems it has had before it for consideration. The project at present under advisement by the Committee is the establishment of a graduate school of tropical agriculture. On the basis of study of a number of possible sites and following visits by the Division Chairman of last year and the present Chairman, the Committee, with the endorsement of the Division, is recommending Porto Rico as the most suitable location for this proposed graduate school.

11. *Committees on Cooperation.*—In the initial stages of the Division's development there was greater need than at present for utilizing every means of bringing to the national

societies a better understanding of the *raison d'etre* and possible usefulness of the National Research Council. To this end, in addition to the regular representation of the Division membership, committees on cooperation were suggested from the societies. For several years these committees functioned and served the purpose for which they were established, but as the Council has become better known to the societies a number of them have been dropped by their respective societies. At present but five such committees are in existence principally because of projects started by them and not yet consummated. It is interesting to note the evolution of these committees on cooperation from merely liason bodies, as was at first conceived, into active working bodies.

On behalf of the Division of Biology and Agriculture, its present officers wish to call the attention of the members of the American Genetic Association to the fact that the National Research Council welcomes the representation on it of the Society and that the Division is the unit through which the Society secures this representation and to whom it may bring any matters in which the Council can be of assistance. The extent to which this Society, along with the others included in this one unit, utilizes the Divsion organization as a means to accomplish some of its needs is the extent to which the Division itself will justify its value to the societies.

A Bequest to the American Genetic Association

The Association has been informed of the death of Mr. E. D. Metcalf, at Buffalo, Wyoming, on December twenty-first. Mr. Metcalf was the author of the will which he brought to the attention of the Association several years ago. He allowed extracts from it to be published in the JOURNAL for November, 1921, though his name was not announced. In this will a fund of $10,000.00 was bequeathed to the American Genetic Association, the income to be used by the Council of the Association for the "increase and diffusion of knowledge regarding heredity." The greater part of Mr. Metcalf's estate was to be set aside as a trust, the income to be used for the support and education of his family. At the end of fifty years, the estate was to be divided—one half to his descendents and one half to the American Genetic Association.

In order that the provisions of his will might be clearly understood Mr. Metcalf embodied in it a brief philosophy of life which set forth with remarkable clarity his belief that the study of heredity represents the only hope of continued progress. It closes with this significant paragraph:

> As a race we have been reared in the lap of nature like other creatures, but the time has now arrived when our destiny is largely in our own hands and we must co-operate with all our strength to eliminate weakness and develop the "high minded man, with powers as far above dull brutes endued as brutes excel cold rocks and brambles rude." Race betterment is the all-important work toward which our resources and energies should be directed, and all other matters are significant only, or mainly, as they contribute to this grand purpose.

The executors of the Metcalf estate inform us that this will was probated on January twenty-eighth. Under the Wyoming Statute the widow has the option of setting aside the will and taking half the estate. The will contained a provision that in the event of the widow's refusing to accept the trust provisions by asserting her legal claim to half of the estate, the other half should revert immediately to the American Genetic Association. When the will was probated Mrs. Metcalf elected to claim this dower right, thus under the terms of the will releasing the other half to this Association. The estate is estimated to be worth more than $250,000.